JUN 1 1923

Vol. VI, No. 3 PSYCHOLOGICAL REVIEW PUBLICATIONS JUNE, 1923

Journal of Experimental Psychology

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PUBLISHED BIMONTHLY BY

PSYCHOLOGICAL REVIEW COMPANY

PRINCE AND LEMON STREETS, LANCASTER, PA., AND

PRINCETON, N. J.

Entered as second-class matter, July 1, 1920, at the post-office at LANCASTER, PA.

Journal of
Experimental Psychology

VOL. VI., No. 3 June, 1923

ADEQUACY OF REFLEX COMPENSATORY EYE-MOVEMENTS INCLUDING THE EFFECTS OF NEURAL RIVALRY AND COMPETITION[1]

BY RAYMOND DODGE

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Photographic records and other data have already been published which proved that *coördinate* compensatory eye-movements maintain an amazingly high level of adequacy under favorable conditions.[2] As far as I know the question of adequacy of *reflex* compensatory eye-movements has never been raised. Any attempt to answer it would have met technical difficulties which for a long time seemed insurmountable. Leaving these technical difficulties for a moment let us define the problem and its bearings.

THE PROBLEM

By the adequacy of compensatory eye-movements we understand the accuracy with which they maintain visual fixation in spite of the bodily movements for which they compensate. In terms of spatial integration the question

[1] The present paper represents part of a research program initiated by the support of the Ernest Kempton Adams Research Fellowship of Columbia University. It is published separately by permission as a part of the research activities of the Committee on Vestibular Research of the National Research Council. The writer takes the present occasion to publicly acknowledge his indebtedness to Mr. S. M. Newhall, formerly assistant in the Psychological Laboratory of Wesleyan University, and to members of his courses in laboratory psychology. This paper presupposes a knowledge of the techniques described in previous publications of this series in the JOURNAL OF EXPERIMENTAL PSYCHOLOGY.

[2] JOURNAL OF EXPERIMENTAL PSYCHOLOGY, 1921, Vol. IV., pp. 257 fol.

of adequacy may be expressed as follows: Under what circumstances, if any, do the vestibular deviations of the eye have any exact spatial relation to rotation? In other words, does the vestibule function as a quantitative spatial receptor for eye reflexes?

On the negative side of the question it is notorious that reflex compensatory eye-movements are frequently quite inadequate. The disappearance of nystagmus during protracted rotation is a case in point. As a natural consequence of the hydrodynamics of the vestibule (peripheral adaptation) vestibular deviation ceases after a few rotations. That is to say, after a few rotations the eye ceases to compensate reflexly no matter how long rotation continues. Analagous decrease in the amplitude and frequency of vestibular nystagmus also appears in consequence of repeated rotations, as the effect of a central adaptation process which we call habituation.

In addition to the inadequacies of vestibular deviation from adequate stimuli, rotation may be sub-threshold with respect to consciousness of rotation and with respect to the related reflexes. When sub-threshold rotation produces no response, compensation is obviously completely inadequate. Compensation may, however, be worse than inadequate. In the nystagmus of deceleration (after-nystagmus), vestibular deviation is diametrically opposed to the requirements of good compensation. In this last case the vestibular deviation of the eyes disturbs vision and general orientation instead of facilitating it. In after-nystagmus the pseudo-compensatory eye-movements are superlatively inadequate.

In view of these known inadequacies our question may appear somewhat academic. It is, however, by no means trivial. The theoretical implications of the question concern the possibility of the spatial adequacy of the vestibular control of movement. The practical implications have a direct reference to the vestibular controls in flying.

Technique

The first condition of a quantitative correlation between the angular velocities of eye-movements and of rotation was a

spatially adequate record of the eye-movements. This desideratum is approached in our present techniques. But unfortunately the exact spatial evaluation of any photographic record of the eye-movements is very difficult. No known method of recording the eye-movements guarantees the correspondence of any point in the record with the visual fixation of any particular point of space. The technical difficulties are not new. The theory of the spatial interpretation of eye-movement records has not advanced beyond the point of our discussion in 'An Experimental Study of Visual Fixation.'[1]

Records of eye-movements from the mirror-recorder are even less well adapted to immediate spatial interpretation than are those of our corneal reflection method or of Judd's kinetographic technique with Chinese white in the eye. The mirror-recorder is, however, the only available instrument for photographing the movements of closed eyes. The main limitations in its use as a spatial instrument may be summarized as follows: (1) Movements of the head correspondingly displace the eye record and complicate it by coördinate compensatory factors. (2) Eyelid movements disturb every record from closed eyes. (3) There is no guarantee that the mirror rests over the axis of the cornea at any moment of the record. If the eye is too high, or too low, or too far to one side, the records will be distorted. While these errors may be negligible in temporal studies of eye-movements, our present problem demands technical reliability of a different order.

For a long time these difficulties seemed to make hopeless any inquiry concerning adequacy of reflex compensatory eye-movements. As far as I can see, absolute measurements of the spatial extent of eye-movements under closed lids continue to be hopeless. Relative spatial measurements offered a way out of a similar difficulty when we were investigating the effects of protracted rotation.[2] The complete adequacy of reflex compensatory eye-movements would

[1] Monograph Supplement of the *Psychological Review*, 1907, Vol. 35, pp. 84-91.

[2] JR. OF EXPERIMENTAL PSYCHOLOGY, 1923, VI., pp. 1-35.

be shown if it could be demonstrated that their angular velocities were identical with the angular velocities of open eyes when looking at an environment which rotated with the same apparent angular velocity. Even with this simplification of the technical problem its solution was far from clear. It would not be satisfactory experimentation to rotate the environment with the subject's eyes open and then to rotate the subject with his eyes closed. Several disturbances might intervene to invalidate comparison of records which were taken under these circumstances. In the first place the eyes always change their position in the socket when both eyelids are closed. It is a well-known but unexplained synergy. With changed position of the eye the recorder would have to be reset before and after each record which was taken when the lids were closed. There would be a consequent loss of quantitative comparability of the records. Furthermore, in such a sequence of records it would be impracticable to maintain absolute fixation of the head. Comparison of records taken under such circumstances would be useless for our purpose.

In view of these difficulties it occurred to us that each rotation might be sub-divided into comparable periods of visual pursuit nystagmus and periods of pure vestibular nystagmus. The former would be registered whenever the subject with one open eye could see his environment through the opening of a diaphragm. Periods of pure reflex compensation would supervene when the diaphragm was closed so that the subject could see nothing of his environment.

This theory determined the reconstruction of the previously described apparatus.[1] The diaphragm was made of thin black sheet metal with an artificial pupil. The shutter which closed this artificial pupil consisted of a semicircle of metal which was mounted on a pulley. It was mechanically synchronized with the movements of the rotating platform by a belt from the speed recording or timing shaft. Each rotation of the platform produced one complete rotation of the shutter. During approximately half of each rotation,

[1] Jr. of Experimental Psychology, 1921, IV., pp. 248 fol.

consequently, the artificial pupil was open and the subject could see his environment. During the remainder of each rotation the artificial pupil was closed and the subject could see nothing but the vague contours of the dark screen in front of him. We made no serious effort to reduce the darkened field to blackness. We are not sure that it would have been desirable. The absence of all clear fixation marks served the purposes of the experiment. The timing shaft which synchronized the shutter with the rotating platform has already been fully described.[1] Essentially, it was a vertical rod geared to the platform so that it stood still in space while the platform rotated around it.

In order to eliminate from the records the results of accidental convergence and divergence of the eyes the natural environment of the subject, that is, the corner of the room in which the rotating platform was set up, was replaced in the visual field of the subject by an artificial environment which consisted of a semicircular cardboard carrying vertical black and white strips.

In order to permit the experimental reversal of movement of this environment the semicircle was attached permanently to the rotating platform and was viewed by the subject in a revolving mirror of four faces. The axes of the revolving mirror and of the semicircular environment were coincident. Like the revolving shutter, the revolving mirror was geared to the timing shaft with a belt. The pulleys were constructed to give the artificial environment the same apparent speed that the real environment would have during rotations of the platform. Disconnecting the belt stopped the revolution of the mirrors and made the artificial environment stationary with respect to the subject. Reversing the belt reversed the apparent movement of the artificial environment. That is to say, instead of rotating with the apparent speed and direction of the natural environment it rotated in the same direction with the platform but twice as fast. This gave us three experimental conditions for comparison. (1) Normal movement of the environment. In this form the visible

[1] JOURNAL OF EXPERIMENTAL PSYCHOLOGY, 1921, Vol. IV., pp. 251 fol.

field moved congruently with the natural environment, *i.e.*, in the opposite direction to the rotation of the subject. (2) Reversed movement of the environment. The second experimental form was provided when the visible environment appeared to rotate in the direction opposite to that of the natural environment. (3) No movement of environment. In the third experimental form the visible environment does not appear to rotate at all. It moved with the subject, *i.e.*, as occurs in the movements of the earth.

The Records

Records of visual nystagmus consequent to pursuit of objects in a moving field alternate with records of pure vestibular nystagmus in Plates I.–IV.

All records in Plates I.–IV. were taken on falling plates and should consequently be read from below upwards. The narrower horizontal bands are time lines two seconds apart. The broader dark bands show the incidence of moments of blindness in which vision of the moving field was cut off by closing the artificial pupil. The light bands conversely represent moments in which the moving field was visible. That is to say, during the light portions of the records the subject saw a moving field. During the broader dark portions of the records no field at all was visible. The device for indicating these moments of vision and of lack of vision consisted of a red-saturated straight filament glow lamp which was fixed in the plane of the camera slit and the eye. The lamp was connected in series with a commutator which was on the axis of the shutter. The commutator was accurately adjusted so that a high resistance paralleling it in series with the lamp, was short-circuited just as the artificial pupil was closed by the shutter and was reintroduced just as the artificial pupil was opened. Tests of the latency of illumination changes under these circumstances showed that they were too short to be measured on our relatively slow records. The incidence of vision of the moving field and of blindness may consequently be regarded as coincident with the beginning and end of the bright horizontal bands.

There are three longitudinal record lines, namely, head-lines, eye-lines, and rotation-indicators. These record lines represent respectively movements of the head, eyes, and the rotating platform. They are marked with appropriate legends on the plates.

All of our records are really negatives, not prints from negatives, consequently, as might be expected, all records appear reversed with respect to left and right directions. Since all are affected in the same way their mutual interrelations remain exactly as recorded. For example, in Plate I., Record 1, the rotation indicator moves off to the right of the record. The actual rotation was consequently to the left or counter-clockwise. The compensatory visual pursuit eye-movements and the vestibular deviation of the eyes go to the left. The real direction was to the right or clockwise. That is to say, the direction of the eye-movements was opposed to that of rotation. Consequently, as the records indicate, the eye-movements were compensatory in their direction.

Form I

Natural Movement of the Visual Field

Comparison of the light and the broader dark phases of these records, *i.e.*, those phases during which vision of the moving field alternated with no vision, shows a relatively smooth series of eye deviations, irrespective of their origin, in Plates I. and II. Particularly in Plate I., Record 2, and Plate II., Record 2, the visual pursuit deviations and vestibular deviations show no measurable differences between the first light band and the first broad dark band. In both of these records the eye-movements of visual and of vestibular origin had the same velocity as is shown by the slants of the eye-deviation records. They had approximately the same amplitudes as is shown by the amplitudes of the eye-deviation records. If I had not known the circumstances under which they were produced I could not have told which was which at this stage of the record. Since the artificial environment was satisfactorily seen during rotation, the visual pursuit

was adequate. Since the vestibular deviation was identical with the visual pursuit, that also must have been adequate. In Plate I., Record 1, and Plate II., Record 1, on the contrary, the vestibular deviations differ from the visual pursuit movements both in amplitude and in angular velocity. This is shown by the fact that the vestibular deviations during the first dark period are not parallel with the visual pursuit deviations. Moreover, they have a smaller amplitude. Since these vestibular deviations have less angular velocity and less amplitude than adequate visual pursuit deviations, they are correspondingly inadequate.

Generalizing from these data one might say that, during rotation, pure vestibular deviation may be identical in character with adequate visual pursuit deviation. Under these circumstances the vestibular deviation is adequate, but the adequacy is neither necessary nor universal.

After the first alternation of light and broad dark phases of the record the pursuit deviation and vestibular deviation always show very different courses. As the rotation continues the vestibular nystagmus gradually decreases in amplitude and frequency until it almost or quite disappears before the rotating platform comes to rest. Comparison with the corresponding visual pursuit records shows that the vestibular deviation disappears before the actual stopping of the platform. That is to say, while vestibular deviation may, on occasion, be an adequate compensation at the beginning of rotation, as rotation continues the vestibular deviation gradually dies out to the complete inadequacy of a resting eye.

Plate I., Record 2 gives an unusually exact picture of the reversal of the vestibular nystagmus incident to stopping. The rotation indicator may be seen crossing the plate obliquely as the rotation slows down just before stopping. The actual stop is not recorded on the plate. The record was cut off to save space. The moment of stopping, however, is indicated by an arrow. The last apparent deviation of the eye-line to the right is due to the mechanical jar of deceleration as is shown by a corresponding deviation in the head-line. Two thirds of a second before the rotation actually

stops, the diminished vestibular deviation reverses its direction quite sharply as it passes into the nystagmus of deceleration, the so-called 'after-nystagmus.' This record constitutes perfect documentary evidence that the so-called 'after-nystagmus' is really a nystagmus of deceleration. A similar reversal due to deceleration is also shown in Plate II., Record 2.

Form 2

Reversed Motion of the Visual Field

The question, what would happen if the visual data conflicted with the vestibular data, seemed particularly interesting from a theoretical standpoint. Will the visual requirements dominate the reflex or will the reflex control of the eyes overbalance the visual requirements? Which will dominate the final common path to the eyes, the cortex or the basal ganglia? That was the question.

In our preëxperimental analysis there seemed to be about equal grounds for expecting both events. There appeared, moreover, a third possibility, that competition for the final common path to the eye might disturb both processes. The experimental records are clear and unmistakable. Their instructiveness relative to the mechanisms of compensatory eye-movements is unique. They are also unusual in their clear portrayal of rivalry of two different systems of control in their competition for a final common path. Plates III. and IV. represent this experimental conflict. Even superficial comparison of these records with Plates I. and II. shows a notable difference. Instead of a relatively smooth succession of oblique lines which indicate congruent deviations of visual pursuit and of vestibular origin, the light and dark phases of Plates III. and IV. show deviations in opposite directions with irregularities, hesitations, and reversals. More careful inspection shows a number of significant details. Plate III., Record 1, shows a complete reversal of deviation shortly after the rotation starts, without a corresponding broad dark phase. That is to say, in spite of contrary visual requirements the first reaction to rotation is a compensatory reflex of vestibular

origin. This is interrupted and reversed only after a clearly marked reaction time of approximately a quarter of a second. With more or less hesitation the eye deviation then passes into a visual pursuit-phase in spite of contrary vestibular stimuli. At the moment of darkening the field, however, the visual pursuit immediately gives place to vestibular reflex deviations. This alternation continues to the end of the rotation. In every case the vestibular reflex deviation appears immediately after the field is darkened. In every case the visual pursuit is established only after an appreciable reaction interval corresponding to the relatively slow reaction time of the eye. In this particular record the alternation in the directions of deviation is fairly regular. Close inspection of the second and third white bands of this record shows very slight hesitations and unevenness of the visual pursuit nystagmus. These hesitations and unevennesses are progressively more conspicuous in Plate III., Record 2, and Plate IV., Records 2 and 1. They are most conspicuous in Plate IV., Record 1, which, as the narrowness of the alternating light and dark horizontal bands shows, was the record of most rapid rotation. In this record the onset of rotation produced considerable jar and vibration as is shown by the oscillations of the head-line. This confuses the eye-record and makes it uncertain until the compensatory deviation obtains considerable amplitude, *i.e.*, in the vicinity of the first white band. As the rotating visual field becomes visible there supervenes a remarkably clear picture of conflict and competition for the final common path. The maintenance of visual fixation is badly disturbed. At this velocity of rotation the two rivals are approximately evenly matched. First one and then the other controls until the darkened field suddenly reinstates the reflex. This is the only record in which the reflex phase also shows any considerable impairment. Somewhat less complicated disturbance of the visual phase is shown in the slightly slower rotation of Plate IV., Record 2. From a comparison of these records it seems probable that extreme speeds of rotation in which visual pursuit would be even more difficult or impossible might show very different

configurations of the eye-movement record. Under such circumstances one would expect more and more complete domination by the vestibular reflex.

Form 3

No Motion of the Visual Field

No records of the effect of rotation on the fixation of a motionless visual field (Form 3) are presented in this paper. This is partly because we are printing a number of plates, which is already large, and partly because the records which we have of Form 3 are relatively uninstructive.

At speeds of rotation analogous to those already reported rotation always produced vestibular deviations in spite of the effort to fixate a motionless field. The only proved effect of fixation of a motionless field on the nystagmus of rotation is reduced amplitude of the vestibular deviations. Whether or not this is accompanied by reduced angular velocity, available records are not adequate to show.

An arrangement to secure quantitatively comparable records with and without fixation of the motionless field has been planned. It is analogous to the arrangement used to secure the records that have just been discussed. Unfortunately, it was planned too late to be used in the present investigation.

Interpretations

Our interpretation of the recorded facts may be summarized as follows: The vestibular reflex deviation of the eyes precedes and initiates all compensatory eye-movements incident to rotation, even when the eyes are open and when they see the visual field. This initial reflex deviation is always in the direction of compensation and may be approximately adequate to compensate for the actual speed of rotation. As a rule, however, the angular velocity of adequate compensation is only roughly approximated in the reflex deviation. Vision of the normal moving field of view transforms this inadequate vestibular reflex into an adequate visual pursuit compensation, as the control of the final

common path to the eyes passes from the basal ganglia to cortical areas.[1] The moment vision is interrupted the reflex control of the final common path is resumed exactly as though it had never been interrupted. At moderate speeds there is no indication of a cortical partnership with or regulation of the reflex. On the contrary the cortex supplants the reflex in control of the final common path to the eyes and is in turn supplanted when vision is interrupted. This alternation of controls is strikingly shown in those records where the vestibular reflex was opposed by an unnatural movement of the visual field.

During unnatural rotation of the visual field at moderate speeds of rotation the visual system assumes control of the final common path after an obvious reaction latency and a moment of conflict. When the visual stimuli to pursuit disappear the vestibular system immediately assumes control as though its domination had never been interrupted. This alternation may be repeated three times at least, that is to say, it lasted as long as our experimental rotation continued.

At moderate speeds of rotation the two processes suffer no confusion. They alternate almost mechanically. As the vestibular stimuli become most intense, *i.e.*, at greater speeds of rotation, with increasing difficulty of adequate visual pursuit, cortical control is established with increased hesitation. At a speed of one rotation in three seconds rivalry becomes conspicuous and both the vestibular and visual controls were impaired. There is, however, at this speed a much less relative impairment of the reflex deviation than of the pursuit deviation. The latter is quite confused. Nevertheless, the fact remains that something unusual happened to the vestibular deviation in Plate IV., Record 1.

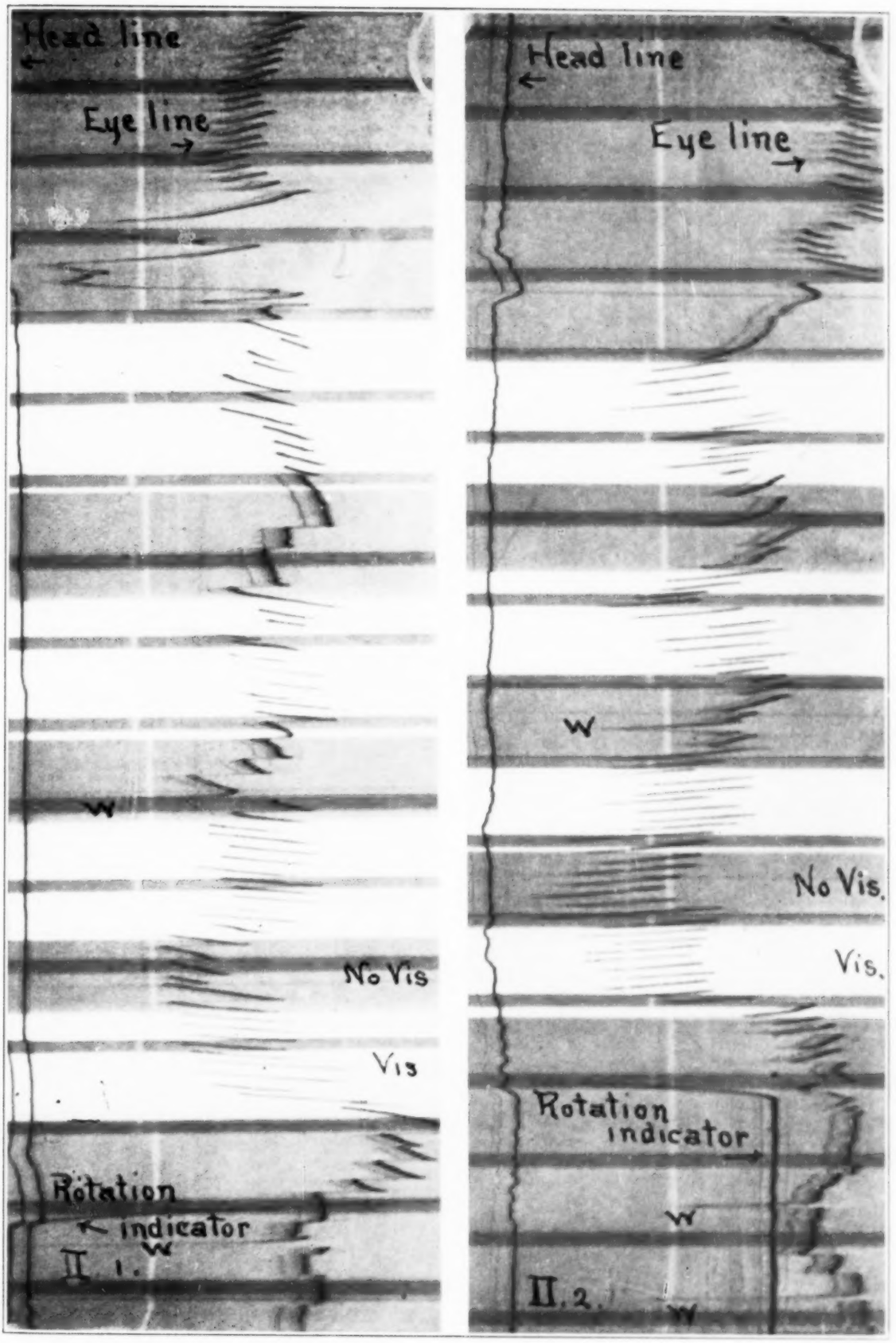
The effort to fixate a motionless field does not interrupt the reflex vestibular deviation in the same manner that visual pursuit interrupts it. Its only effect was an apparent re-

[1] An analogous, quick, but rough adjustment in the direction of probable utility occurs in such reflexes as the knee-jerk. Real adequacy of response as in the vestibular reflex depends on the assumption of control by the cortex with its more completely elaborated data.

Record 1 Record 2

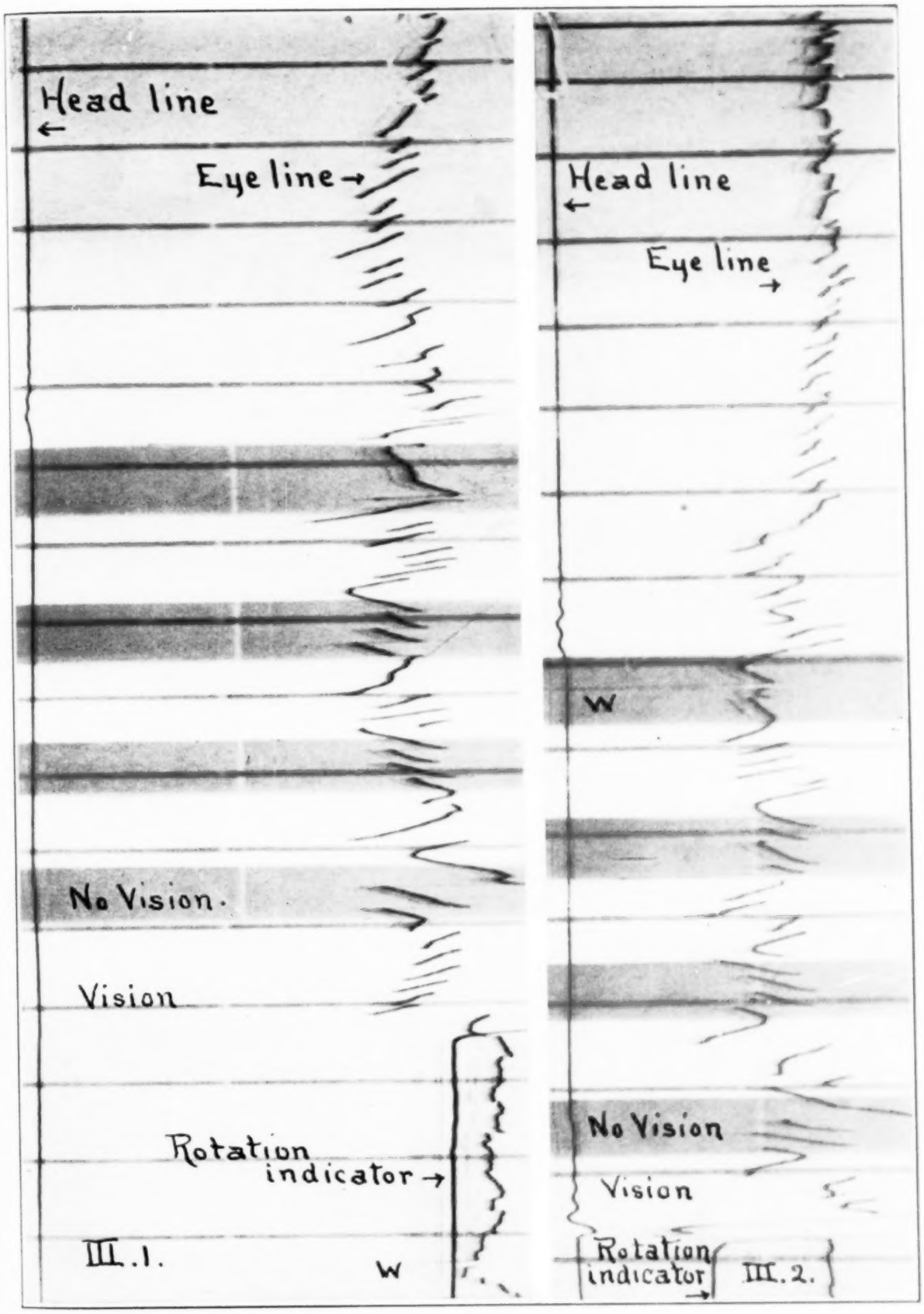
PLATE I

Plates I. and II. show the combination of visual pursuit and vestibular nystagmus. They show the degree of adequacy of the latter as a form of compensation.



Record 1 Record 2

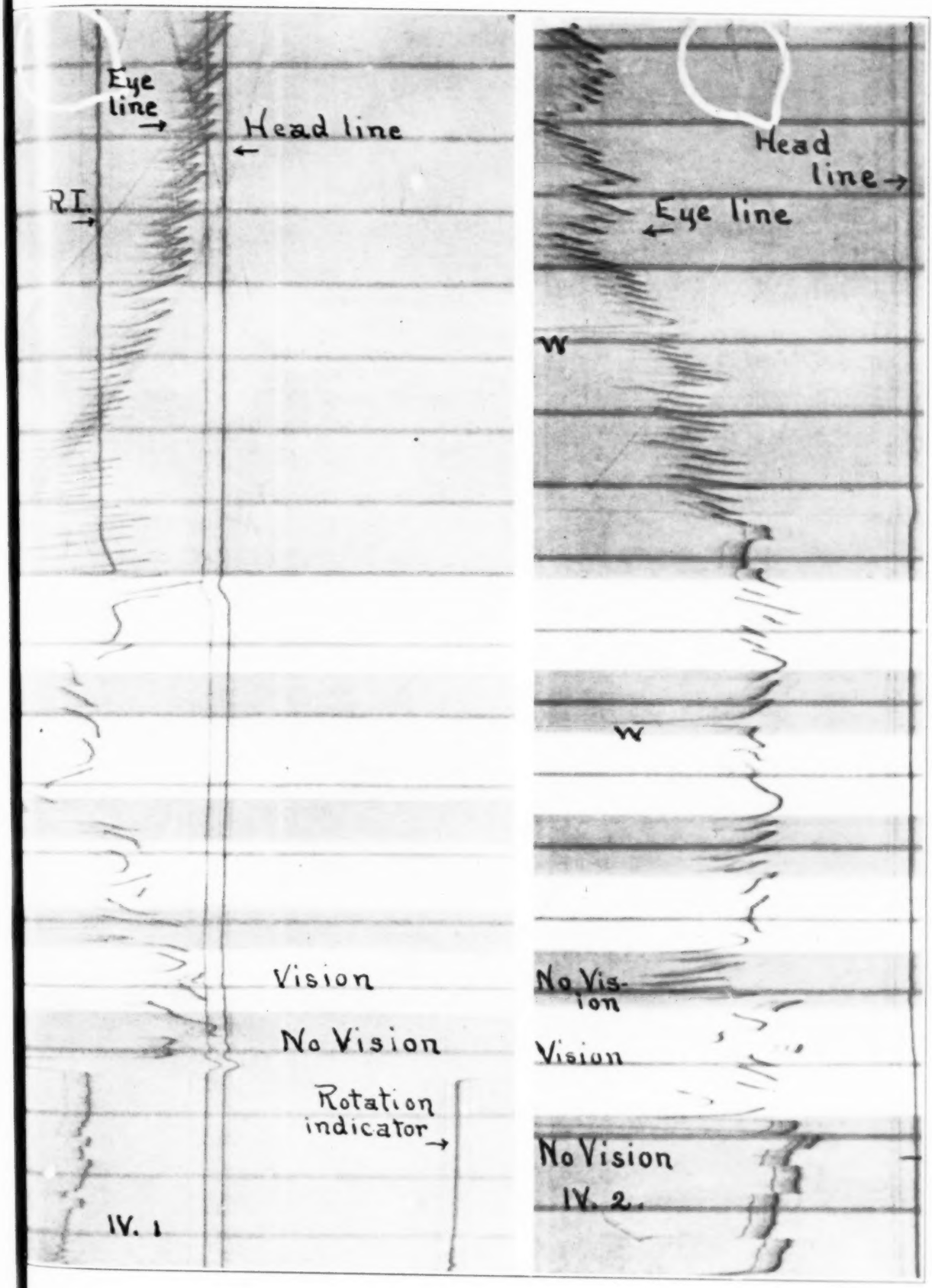
PLATE II



Record 1 Record 2

PLATE III

Plates III. and IV. show the conflict between visual pursuit in one direction and vestibular nystagmus in the other.



Record 1

Record 2

PLATE IV

duction in the amplitude of vestibular deviations. Whether it also decreases the angular velocity of the reflex deviations of acceleration and deceleration our present records do not give satisfactory data for determining.

All of our records agree that the vestibule is not a quantitatively exact regulator of action. It reflexly initiates with very low reaction latency a useful compensation of the eyes which is occasionally entirely adequate but whose adequacy depends on the subsequent control of vision. Quantitative unreliability of vestibular controls of action must be considered in any estimate of their possible role in flying.

In view of the technical completeness of these records one may perhaps be pardoned for exceeding the limits of description and direct interpretation to point out their implications as unusually clear pictures of rivalry and conflict in the control of the final common path as phenomena of our complex nervous systems. We believe that the present pictures of rivalry between the higher and lower systematizations and of alternating control of the final common path with possible impairment of both controls in extreme cases offer a striking and instructive view of human neural mechanisms. How these phenomena of conflict will be affected by habituation and whether they appear in other systematizations of the human nervous centers are still problems for experimental investigation.

These rivalries and alternations in control may reasonably be expected to run a very different course in consequence of impairment of the relevant areas in the cortex or in the basal ganglia. It seems probable that they might prove of high diagnostic value if reliable techniques could be reduced to clinical simplicity.

CONSTANCY OF AFFECTIVE JUDGMENT TO ODORS

BY PAUL THOMAS YOUNG

University of Illinois

The problem of the following experiment is to determine the degree of constancy of affective judgments to odors and to discover, if possible, the conditions of variation.

Eight odors were carefully selected. The experimenter aimed to select four pleasant and four unpleasant smells with different degrees of affective intensity. Every precaution was taken to keep the odors constant throughout the course of the experiment. There were three experimental hours a week either on M. W. F., or T. Th. S., and the experimental hour for each subject was the same on every day. The experiment lasted for five weeks. The subjects were two men (*A* and *C*) and two women (*B* and *D*) all connected with the department of psychology at the University of Minnesota (November–December, 1920).

The method of an actual experiment was as follows. The subject seated himself upon a pillow on a table, his legs hanging freely. He was given the following typewritten instruction:

> Be passive, indifferent and relaxed throughout the entire experimental period.
>
> Simple odors will be presented.
>
> When you hear the signal 'all right' report immediately whether the preceding experience was pleasant, unpleasant or indifferent; and indicate the intensity of the feeling aroused, by using such words as *very* (P or U); *moderately* (P or U); *weakly* (P or U); indifferent.
>
> Report anything further which you consider significant.

After reading the instruction the subject closed his eyes and the experimenter selected a vial, from a table behind a screen, and placed it approximately two centimeters beneath the subject's nose. When the subject had taken two full inspirations the experimenter gave the signal 'all right,' at the same

time removing the odor. The affective judgment and the complete report were recorded.

The odors were numbered, 1 to 8, and presented in an haphazard order.[1] A period of half an hour intervened between the first four and the last four stimulations, this time being given to another experiment.

The raw data are shown in Tables I.–IV. For convenience in representing the results the following arbitrary scale has been adopted:

+3 very pleasant
+2 moderately pleasant
+1 weakly pleasant
0 indifferent
−1 weakly unpleasant
−2 moderately unpleasant
−3 very unpleasant

Occasionally the subject would use such a phrase as 'more than moderately pleasant,' and to represent these judgments intermediate scale values have been used. For example, +2.5 indicates that the feeling intensity was reported as between moderately and very pleasant.

In these tables, two figures $(^{+1}_{-1})$ indicate a change of quality and intensity during the course of stimulation. The upper figure shows the first feeling and the lower figure, the second to which a transition was made.

[1] The fifteen haphazard series were as follows. Numbers refer to the odors (see Tables I. to IV.).

Series Number	Odor Numbers	
1	5 7 6 8	1 4 3 2
2	2 5 6 7	1 8 4 3
3	5 7 2 3	1 8 4 6
4	1 8 2 4	7 3 5 6
5	2 6 4 5	8 1 3 7
6	8 4 3 1	6 2 5 7
7	3 1 7 2	8 6 4 5
8	2 7 3 4	8 5 6 1
9	3 7 2 8	5 6 4 1
10	7 4 5 8	1 6 2 3
11	5 4 6 2	3 1 7 8
12	4 7 6 3	5 1 2 8
13	2 6 1 8	4 7 5 3
14	3 6 1 5	8 2 4 7
15	3 5 7 6	4 1 2 8

TABLE I

SUBJECT *A*

Odor	Day Number															Av.	M.V.
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15		
1. Oil of bergamot	+2 −1	+3	+2	+3	+2	+1 −1	+3	+2	+2	+3	+3	+2	+2 −1	+3	+2	+2.1	0.7
2. Caproic acid.....	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−2	−2.9	0.1
3. White rose perfume .	+3	+3	+3 −1	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+2	+2.8	0.3
4. Oil of anise....	+3	+2	+2	+2.5	+3	+2	+3	+3	+3	+3	+3	+2	+3	+2	+3	+2.6	0.5
5. Spirits of camphor.	+1	+3	+3	+3	+3	+3	−1 +1	+3	+3	+3	+3	+2	+3	+3	+2	+2.5	0.7
6. Creosote.	−1	−3	−2	−2	−1	−2	−2	−2	−2	−1	+1	+1	+1	0	0	−1.0	1.1
7. Cod liver oil.....	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−2	−3	−3	−3	−2.9	0.1
8. Asafœtida.....	−1	+1 −2	−2	−2	−2	−2	−2	−2	−2	−1	−2	−1 +1	−1	0	0	−1.3	0.7

Some of the conditions underlying these intensive and qualitative shifts are indicated by the reports of the subjects. *A* noted that the intensity of feeling varied with the depth of inspiration, and that P, during inspiration, sometimes changed to U, during expiration. *A* and *B* reported occasionally that a P odor became U owing to the appearance of a sickish or nauseating quality. *D* stated that recognition of an odor increased its P. All of the subjects, during the early part of the experiment, mentioned associations which modified the final judgment.[1]

[1] The experimenter aimed to get a report of the quality and intensity of affection aroused *by odors* during the period of stimulation. Feelings obviously due to conditions other than the odors have been ignored in making the tables.

It is not always possible to assign to a report a single value; even two values do not adequately picture the experience affectively. In computing the averages which are shown in the tables a single intermediate value was used for cases in which there was a shift during the period of stimulation.

TABLE II

SUBJECT *B*

Odor	Day Number															Av.	M.V.
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15		
1. Oil of bergamot ..	+3	+2	+2	+2	+2	+3	+2	+2	+2	+3	+2	+2 −1	+2	+2	+2	+2.1	0.4
2. Caproic acid...	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3.0	0.0
3. White rose perfume	+1	0	+1	+1	+1 0	+2	+2	+1	+2 −1	*x*	−1	+1	+1	+1	*x*	+0.8	0.5
4. Oil of anise ..	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+2	+3	+3	+2.9	0.1
5. Spirits of camphor...	+2	+1	+2	+2	+2	+2	+1	+2	+2	+1	+1	+2	0	+2	+3	+1.7	0.6
6. Creosote ...	+2	+2	+1	+2	+1.5	+1	+1	+1	+1	+2	+1	0	+1	+1	+1 0	+1.2	0.5
7. Cod liver oil	−2	−2	−2	−2	−3	−3	−3	−3	−3	−3	−3	−3	−2	−3	−3	−2.7	0.4
8. Asafœtida....	0	−1	−1	−1.5	?	+1 −1	0	0	0	0	0	0 −1	−1	0	0	−0.4	0.5

If the reader will run his eye along the rows of figures in Tables I.–IV., he will discover that some of the odors are judged every day at the same intensity of feeling. For *B*, caproic acid is always 'very U.' For *C*, oil of anise is 'very P,' and asafœtida 'very U.' There are also series of affective judgments which are perfectly constant except for a single varying judgment. Examples are: *A* (Table I., odors 2 and 7) caproic acid and cod liver oil; *B* (Table II., odor 4) oil of anise; *C* (Table III., odor 5) spirits of camphor; *D* (Table IV., odor 2) caproic acid. Several judgment series have decided constancy but are broken by two, three or four varying judgments. Examples are: *A*, 3; *B*, 1; *C*, 6; *D*, 3,

There are several cases of doubt (?) all but one with *D*, and three judgments (*x*) which had to be thrown out of the data owing to the great complexity of the affective experience.

7, 8. In the more irregular series there is a marked tendency for the same report to occur for several successive days. Illustrations may be found in: *A*, 4, 6, 8; *B*, 3, 5, 6, 7; *C*, 1, 3; *D*, 1, 6. There are a few series which are markedly

TABLE III

SUBJECT *C*

Odor	Day Number															Av.	M.V.
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15		
1. Oil of bergamot	+1	0	−1	+0.5	+2	+2	+1	+1	+1	+1	+1	+1	+1	+1	+1	+0.9	0.4
2. Caproic acid.....	−2	−1	−2	−3	−1	−1	−3	−1	−3	−2	−3	−3	−2	−1	−3	−2.1	0.7
3. White rose perfume .	+3	+3	+3	+3	+3	+3	+2	+2	+2	+2	+3	+2	+3	+2	+1	+2.5	0.6
4. Oil of anise....	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3	+3.0	0.0
5. Spirits of camphor.	+1	+1	+1	+1	+1	+1	+1	+2	+1	+1	+1	+1	+1	+1	+1	+1.1	0.1
6. Creosote.	−1	−1	−2	−2	−1	−2	−1	−1	−1	−1	−1	−2	−1	−1	−1	−1.3	0.4
7. Cod liver oil.....	−1	−2	−2	−2	−3	−3	−2	−3	−2	−2	−3	−2	−1	−2	−1	−2.1	0.5
8. Asafœtida.....	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3.0	0.0

variable but the variations are within the same affective quality: *C*, 2, 7; *D*, 5. There are thus various degrees of constancy. Perhaps the most irregular series is *D*, 1 which shows a great deal of doubt along with the variation.[1]

[1] Major, experimenting with colors, notes seven classes of series:

1. Perfectly regular affective series.
2. Affective series becoming regular as experimentation progressed.
3. Series varying within the same affective quality.
4. Regular series abruptly broken by a single term.
5. Progressive and regressive affective series (rare).
6. Series of indifference
   (*a*) regular non-affective series.
   (*b*) vacillations round the non-affective judgment.
7. Irregular series.

Major, D. R., 'On the Affective Tone of Simple Sense Impressions,' *Am. Jour. of Psychol.*, 7, 1895, 63–67.

TABLE IV

SUBJECT *D*

Odor	Day Number															Av.	M.V.
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15		
1. Oil of bergamot	+3 −1	+3	+1 −1	−1	−1.5	?	+1	?	−1	−1	?	?	−1	−1	−1	−0.2	1.1
2. Caproic acid	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3.5	−3	−3.0	0.0
3. White rose perfume	+3	+3	+3	+3	+2	+2	+3	+3	+3	+3	+3	+3	+3	+3	+3	+2.9	0.2
4. Oil of anise	+2 −2	−2 −3	?	−2.5	−3	−2.5	−2.5	−2	−2	−2.5	*x*	−1.5	−2	−2.5	−2	−2.1	0.5
5. Spirits of camphor	+3	+2	+2	+3	+2	+2	+1.5	+2	+2	+1 +2	+2.5	+2	+2	+2.5	+2.5	+2.2	0.4
6. Creosote	+3 0	+3	+2	−1	+2 +1	+1 +2	+1 +2.5	+2.5	+2.5	+1	+1	+1	+1.5	−1	+2	+1.4	0.8
7. Cod liver oil	−1	−2	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−3	−2.5	−3	−2.8	0.3
8. Asafœtida	−2 −3	−3	−3	−3	−3	−2	−2	−3	−3	−3	−3	−2.5	−3	−3	−3	−2.8	0.3

There is not much evidence for progressive and regressive series of judgments, but perhaps such evidence would appear if the experiment were of longer duration. Two of the series (*A*, 6 and 8) seem to approach indifference on the last six days. The first of these (*A*, 6) makes a curious mutation from consistent U to P on days 11, 12, 13. The judgments upon one odor (*C*, 1) show a good deal of variability for the first six days and then settle down to perfect constancy for the remainder of the experiment. The same odor evoked doubt with *D*, but *D* also shows a 'settling down tendency' on the last three days of the experiment (*D*, 1). In general, the changes of judgment are abrupt [1] and not progressive shifts (note *C*, 3).

TABLE V

AVERAGES IN ORDER OF MAGNITUDE

Subject *A*			Subject *B*			Subject *C*			Subject *D*		
Odor Number	Av.	M.V.	Odor Number	Av.	M.V.	Odor Number	Av.	M. V.	Odor Number	Av.	M. V.
3	+2.8	0.3	4	+2.9	0.1	4	+3.0	0.0	3	+2.9	0.2
4	+2.6	0.5	1	+2.1	0.4	3	+2.5	0.6	5	+2.2	0.4
5	+2.5	0.7	5	+1.7	0.6	5	+1.1	0.1	6	+1.4	0.8
1	+2.1	0.7	6	+1.2	0.5	1	+0.9	0.4	1	−0.2	1.1
6	−1.0	1.1	3	+0.8	0.5	6	−1.3	0.4	4	−2.1	0.5
8	−1.3	0.7	8	−0.4	0.5	2	−2.1	0.7	7	−2.8	0.3
2	−2.9	0.1	7	−2.7	0.4	7	−2.1	0.5	8	−2.8	0.3
7	−2.9	0.1	2	−3.0	0.0	8	−3.0	0.0	2	−3.0	0.0

In Table V. the average scores are ranked in order of magnitude from +3 to −3 and the mean variation for each average is shown. The table shows clearly that, for all subjects, variability is least at the two extremes of the scale.

[1] Some of the conditions which may account for these mutations are: (1) Successive affective contrast. Several reports indicate that a U may be felt more intensely U by contrast with the preceding experience. (2) Temporary mood. There is definite evidence that a cold reduces the intensity of the smell and in this way changes the affective experience. (3) Associations, present at the start of the experiment but dropping out as the experiment progresses, account for some of the variations. (4) Recognition of the odor may change the total feeling. Surprise, attentive strain, fatigue, and other conditions of this sort possibly modify the final judgment.

For subjects *A*, *B* and *D* the M.V.s make a fairly regular curve with the peak near the region of indifference and a marked drop at the extremes of the scale. The curve for *C* is bimodal with a drop in the region of indifference.

How shall we interpret the finding that daily variations in affective judgment are greatest in the region of indifference and least at the extremes of intensity? It may be that the intense feelings are actually variable, but that a scale with only three degrees of U and three degrees of P is not adequate to the intensive differences. *D* states that "there are lots of degrees of 'very U.'" *A* distinguishes between 'very U' and 'intensely U.' But these differences have been obscured by placing all 'very U's' into the same class, −3. This limitation of the scale may account, in part, for the finding.

Probably, however, the intense affections *are* stable, and because relatively free from the variable conditions which bring about frequent shifts of judgment with the weak affections.[1]

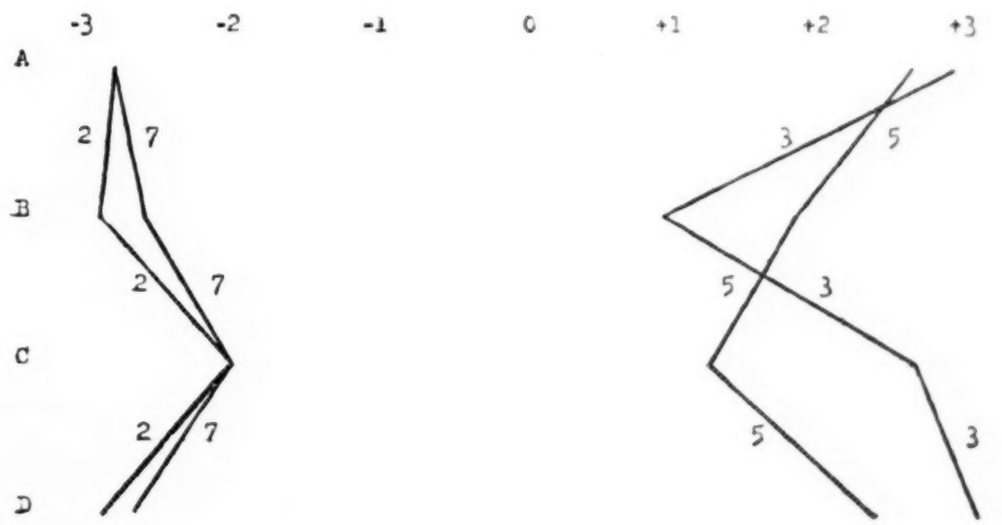


FIGURE I

[1] This hypothesis is confirmed by a recent study of Washburn and Grose in which the problem was to vary likes and dislikes voluntarily. "As was to be expected, it was thus found that *extreme judgments are harder to change voluntarily than moderate judgments.*" This work was done with colored papers.

Washburn, M. F., and Grose, S. L., 'Voluntary Control of Likes and Dislikes; the Effect of an Attempt Voluntarily to Change the Affective Value of Colors,' *Am. Jour. of Psychol.*, 32, 1921, 284–290.

In order to see how the subjects differ in their affective judgments, the average scores have been plotted, Figs. 1 and 2. It is shown in Fig. 1 that odors 2 (caproic acid) and 7 (cod liver oil) are more than moderately unpleasant for all subjects. Odors 3 (white rose perfume) and 5 (spirits of camphor) are judged pleasant by all subjects, but there is more variation among individuals than in the case of the former two odors.

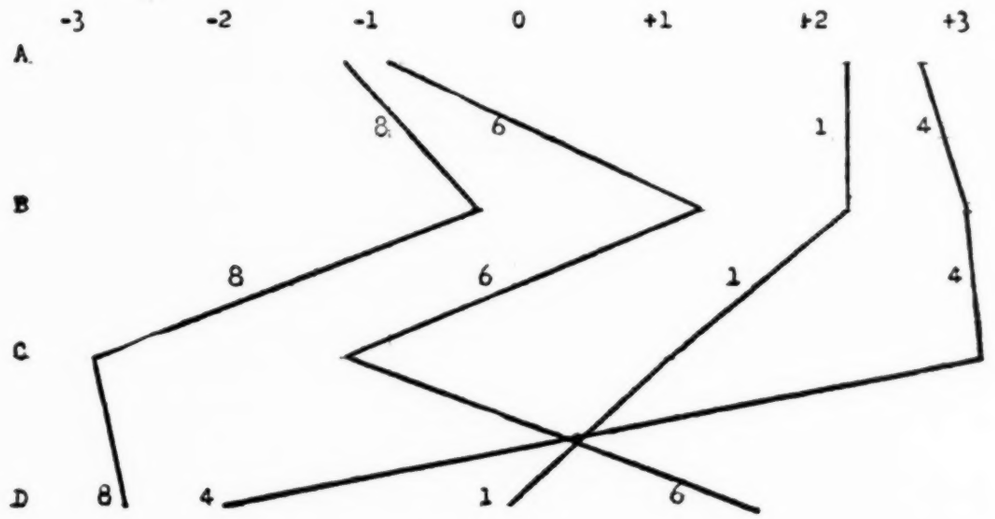


FIGURE II

The judgments shown in Fig. 2 show greater individual differences than those in Fig. 1. Number 8 (asafœtida) is U for all subjects, weakly U for *A* and *B*, strongly U for *C* and *D*. Number 6 (creosote) is the most indifferent odor of the eight. It is weakly U for the two men, *A* and *C*, and weakly P for the two women, *B* and *D*. Number 4 (oil of anise) is very P for *A*, *B*, *C*, but moderately U for *D*. Number 1 (oil of bergamot) is likewise P for *A*, *B*, *C* and just barely U for *D*.

The subjects agree about the quality of five out of eight odors: 2, 7 and 8 are U; 3 and 5 are P. Two of the remaining three odors (1 and 4) are judged P by all the subjects except *D*. One odor (6) is weakly P for two subjects and weakly U for the other two.

Associative tendencies [1] probably account for some of the

[1] Nakashima distinguished between direct and associative judgments. Direct judgments he again divided into '(1) judgments prompted by the tones themselves,

individual differences. *D*, for example, repeatedly stated that oil of anise (4) suggested licorice candy which she disliked greatly. *A* and *C* reported that creosote (6) brought up unpleasant memories. For *A* it suggested hospital experiences and it was sometimes accompanied by a slight nausea. It is interesting to note that *A* reported this odor weakly P on days 11, 12 and 13 (probably when associations had dropped out).

Possibly individual differences in olfactory acuity account for some of the variations. *B* frequently stated that odor 3 (see Fig. 1) is weak and indefinite, and this odor, also, was judged more nearly indifferent by *B* than by the other subjects. Again, the general attitude of the subject in making the judgments is exceedingly important. *D's* attitude toward odor 1 (see Fig. 2) was quite consistently one of doubt and uncertainty. Probably this series of judgments should be thrown out entirely on the ground that the experimental attitude was not comparable to that for the other odors.

Summary

Eight odors were presented to four subjects three times a week for five consecutive weeks and a record kept of the affective judgments. The records show that the quality and intensity of feeling may vary during the period of stimulation (two inspirations). The series of affective judgments show various degrees of constancy. The most variable odors are in the region of indifference while the most constant are either 'very pleasant' or 'very unpleasant.' The same judgment may be made for several successive days and then vary by an abrupt change. There is very slight evidence of progressive and regressive changes in judgment. The four subjects agree fairly well in their affective judgments upon odors, and some of the greatest individual differences can be explained on the basis of associative tendencies.

with or without certain concomitant processes (especially organic sensations), and (2) judgments prompted by other motives than the tones.'

Nakashima, T., Contributions to the Study of the Affective Processes,' *Am. Jour. of Psychol.*, 20, 1909, 168.

SOME FACTORS DETERMINING THE ORDER OF ELIMINATION OF CULS-DE-SAC IN THE MAZE

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The Problem

While conducting an experiment [1] with the white rat in the learning of a complex maze, it was observed that certain culs-de-sac seemed to offer comparatively little difficulty to the animal, whereas certain others were entered many times oftener and were almost invariably the last ones to be eliminated. In fact, the final score, in the case of practically every animal of the group, was associated with either one or another of three culs-de-sac from among the nine included in the particular maze employed. Moreover, the difficult culs-de-sac were observed to be of the same type, while the easier ones were of a markedly different type.

These observations suggested the possibility that a detailed analysis of the error data obtained in the experiment might throw some light upon two closely related problems: (1) As to whether or not there is any consistent relation between the serial position of the various culs-de-sac with respect to the foodbox or goal of the maze and the temporal order of their elimination. (2) As to the relative difficulty of various types of cul-de-sac and the explanation of differences in difficulty in terms of the known reactive tendencies of the animal.

The evidence so far reported upon the first topic has been more or less contradictory and inconclusive. Vincent (8) found a marked tendency for the white rat to enter the culs-de-sac in the last half of the maze less often, in which case

[1] Warden, Carl John, The Distribution of Practice in Animal Learning, *Compar. Psy. Monog.*, Jan., 1923, No. 3.

they would naturally be eliminated earlier, than those located in the first half. Corroborative results were obtained by both Carr (1) and Peterson (6), all three of the investigators mentioned having used more or less similar modifications of the square, or Hampton Court maze. The results of Hubbert (4) on the Watson circular maze indicate the same tendency to backward elimination of culs-de-sac, although such a sequence was by no means invariable. Later Hubbert and Lashley (5) found no tendency to the regressive order of elimination in a maze of similar pattern, and conclude that probably the rate of learning a given reaction is not affected by its temporal relation to the situation of foodgetting. Such diversity of results as to the empirical facts in the case indicates clearly the need of gathering further data upon a wider variety of maze patterns. It is not at all impossible that the order of elimination of culs-de-sac obtaining in a given case may be very largely a function of the type and arrangement of the culs-de-sac in the specific pattern investigated.

Concerning the second problem suggested at the beginning, involving the study of the reaction of the animal to different types of cul-de-sac, only a relatively small amount of experimentation has as yet been reported. Peterson (7) made an extensive study of the factor of length of cul-de-sac and found that the shortening of the particular type studied to the extent of one half brought about a reduction of from 29 per cent. to 47 per cent. in the total number of entrances made during the period of learning. That a short cul-de-sac offers less difficulty than a longer one, other things being equal, may be accounted for, as Peterson seems to think, on the principle earlier pointed out by Carr (2) that the extent of the animal's excursion into a cul-de-sac is gradually decreased during the progress of learning, as well as the actual number of entrances. Hubbert and Lashley (5) found incidentally, in an experiment upon the circular maze, that the error of going past an opening that should be entered was eliminated with approximately one third less trials than was the error of selecting the wrong direction after passing through the opening. The explanatory suggestion is offered that perhaps the white rat has a natural

tendency to enter openings, in which case the problem of selecting the proper direction may offer at once the more difficult and more typical trial and error situation of the two. Dashiell (3) analyzed the choices made during the first trial by fifty white rats at cul-de-sac openings located differently with respect to the true pathway, with the following results: a cul-de-sac opening straight ahead is more likely to be entered than to be passed in the ratio of about five to three, whereas a cul-de-sac opening at the side is about as likely to be entered as to be passed. These few studies serve at least to emphasize the differential character of various types of cul-de-sac situations and to suggest the possibility that a more complete analysis of the maze will throw considerable light upon the rôle played by typical unit coördinations in the formation of complex motor habits.

The Method

In the present experiment an animal maze of the usual construction of wood with metal partitions was employed. The runways were 10 cm. wide and of about the same depth and the length of the true pathway was 810 cm. The general plan of the maze is shown in Fig. 1 in which the culs-de-sac are numbered serially from the entrance to the foodbox, the numbers being used to designate the different culs-de-sac throughout the later discussion. The entire apparatus was painted a dull black, and intervals of 10 cm. were marked off along the runways by means of narrow white lines in order to aid the experimenter in making pencil tracings of the gross movements of the animal, for each separate trial, upon small printed patterns of the maze.

From these tracings the number of entrances made into each cul-de-sac were checked, together with the direction of orientation of the animal at the time of entry. Only forward entrances, however, have been included in the tables because obviously these alone actually determine the temporal order of elimination. All entrances into culs-de-sac occurring during returns, when the animal is oriented toward the entrance of the maze, are eliminated at an early stage of the

learning process by virtue of the fact that the animal soon establishes the habit of continuing persistently the forward direction. Thus all are eliminated practically simultaneously and without the necessity of building up a definite avoidance reaction pattern in each case, as is true of the forward cul-

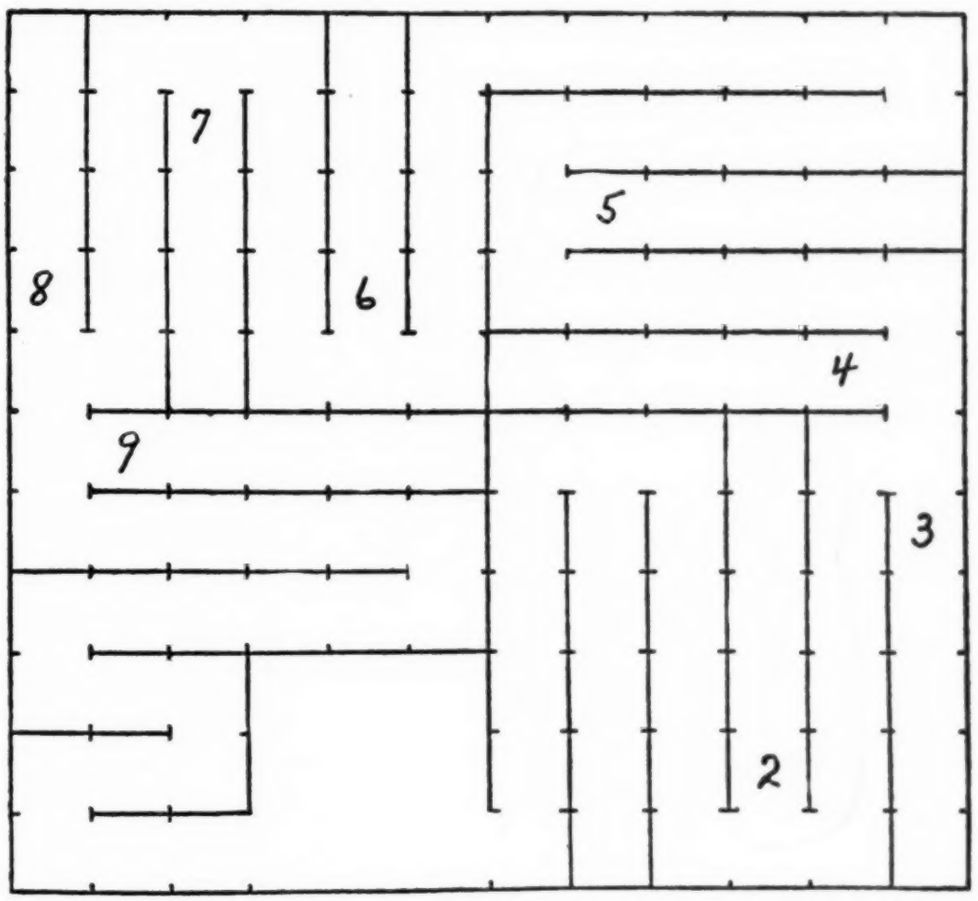

FIG. 1

de-sac errors. Return errors, whether involving entrance into a cul-de-sac or merely the retracing of some section of the true pathway, are to be associated with the period of initial adjustment [1] of the animal to the total maze situation previous to the dominant functioning of the hunger stimulus, and are wholly unrelated, therefore, to the problem of the final order of cul-de-sac elimination. The habit of retracing during the earlier trials does tend to increase, however, the

[1] Warden, *op. cit.*, especially page 42 *ff.*

number of forward entrances into culs-de-sac, inasmuch as it involves the possibility that certain culs-de-sac, particularly those near the entrance of the maze, may be entered more than once during a given trial. Forward errors thus occasioned by returns have been subtracted from the total number of cul-de-sac errors and are given separately in Table IV, and the values there indicated may be considered as indices of the effect of retracings in determining the order of elimination of culs-de-sac in the maze pattern here used.

The development of a highly smooth and automatic reaction pattern at each cul-de-sac was required. Elimination was construed to mean the final dropping out of any observable tendency to enter a cul-de-sac, and such an act as merely extending the nose a slight distance into an opening in passing was accordingly checked as an error. This is in somewhat sharp contrast to the method of counting errors often employed in which an error is checked only when the animal has actually entered the cul-de-sac for a distance equal to or greater than its own body length, and it is of course possible that the data here reported are, in part, a function of the criterion of learning employed. The maze as a whole was considered learned when nine trials out of ten were made without error. In computing the number of trials required to eliminate a given cul-de-sac, a like standard for the individual cul-de-sac was used.

Results

The individual records of the 35 animals as to the number of trials required to eliminate each of the nine culs-de-sac of the maze are given in Table I. At the foot of the table the group average for each cul-de-sac is indicated, together with the ordinal rank of each as to relative speed of elimination. A comparison of the values in the table for the different culs-de-sac will show that there is no tendency whatever to a regressive order of elimination. It will be seen that this is true not only for the group average, but also for practically every individual record of the group. The position of the cul-de-sac with respect to the foodbox, therefore, if a factor

at all in determining the relative speed of elimination, would seem to be entirely negligible in the learning of a maze of this pattern. Culs-de-sac 3 and 8 of the series as numbered in Fig. 1 are the first two eliminated, while numbers 4 and 9 with practically tied values are next in order. Numbers 3 and 4 are, of course, relatively near the maze entrance, while numbers 8 and 9 are the two nearest to the foodbox. The total lack of any serial order with respect to the goal end of the maze in the matter of elimination is shown by the fact that numbers 4, 5, 6, 7 and 9 are all eliminated later than number 3 which is located in the first section of the maze, while numbers 5, 6, 7 and 9 are likewise all eliminated later than number 4. Furthermore, if the averages for the first four, be compared with the averages for the second four culs-de-sac, it will be seen that somewhat fewer trials were required to eliminate the first four than the second four culs-de-sac of the maze, although the difference in the two cases is hardly significant.

The wide differences in the absolute values support the same general conclusion as that reached on the basis of the ordinal values. For example, cul-de-sac number 3 located in the first section of the maze and sixth in order from the foodbox required an average of only 4.5 trials for elimination. This number was approximately only one third as many as for number 9 located next to the foodbox, one fourth as many as for number 7, one seventh as many as for number 6, one eleventh as many as for number 5, and one third as many as for number 4. In each case the absolute value as to the number of trials required to eliminate should have been smaller than for number 3, instead of from three to eleven times as large, if a regressive order had been followed.

Attention should also be called, in this connection, to the uniformity and consistency of the individual records of Table I in following the averages just discussed. Thus numbers 8 and 3 were both eliminated before any other cul-de-sac by 30 of the 35 animals. And there were very few cases in which the last cul-de-sac to be eliminated was not either number 1, number 5, or number 6. The data throughout seems to fully

TABLE I

RELATIVE NUMBER OF TRIALS REQUIRED TO ELIMINATE THE VARIOUS CULS-DE-SAC

Rat Number	Culs-de-sac Numbered in Order from Entrance of Maze								
	1	2	3	4	5	6	7	8	9
1	37	24	0	14	42	40	34	1	10
2	36	11	5	9	3	21	21	2	8
3	34	48	2	22	34	25	14	2	9
4	46	35	0	10	34	28	27	1	14
5	47	14	7	23	56	15	11	0	23
6	65	10	2	4	23	16	3	0	21
7	72	30	0	8	68	40	30	0	15
8	79	22	12	38	83	60	13	12	28
9	88	25	1	7	50	42	27	3	11
10	86	21	1	14	80	49	7	0	17
11	94	12	5	24	101	63	15	17	12
12	106	12	0	17	101	69	14	1	10
13	47	21	3	21	28	27	14	5	13
14	47	45	3	14	30	29	5	1	21
15	48	41	1	6	39	39	21	2	18
16	46	48	9	30	33	28	45	0	20
17	60	25	0	6	28	18	22	0	13
18	64	35	5	23	21	15	14	0	12
19	63	49	2	1	41	35	25	1	6
20	47	61	4	16	42	50	40	3	21
21	69	59	12	17	73	44	22	6	13
22	76	22	1	15	54	30	34	3	16
23	83	31	1	1	71	50	19	8	19
24	88	70	11	15	67	61	44	4	23
25	24	11	11	8	19	9	10	1	6
26	25	38	2	14	26	14	25	9	27
27	23	32	17	19	46	27	39	1	5
28	38	24	5	18	52	20	9	6	16
29	24	27	1	20	34	27	49	0	32
30	55	25	5	35	61	26	8	0	18
31	24	24	0	16	50	17	26	1	12
32	22	21	8	17	64	18	22	0	28
33	39	9	6	7	64	42	21	0	9
34	44	25	10	24	30	24	33	1	13
35	52	72	4	10	78	27	25	1	10
Group ave.	54.2	31.1	4.5	15.5	49.3	32.7	22.5	2.6	15.7
Order of elimination	9	6	2	3	8	7	5	1	4

warrant the conclusion that the serial position of the culs-de-sac of a maze with respect to the foodbox is not a factor of consequence in determining the temporal order of their elimination, at least in a maze of the pattern here employed.

It is clear, therefore, that some principle other than that of the relative proximity of the cul-de-sac to the foodbox is necessary to account for the order of elimination here obtained. The question naturally arises at this point as to the compara-

bility of the various culs-de-sac included in the maze pattern used, inasmuch as a perfect regressive order could hardly be expected in any case except where all the culs-de-sac were highly similar in type, and thus called for an approximately identical type of motor coördination. A careful examination of Fig. 1 will show that the nine culs-de-sac of this maze may be grouped into three classes as follows: type 1—numbers 3 and 8, type 2—numbers 4 and 9, type 3—numbers 1, 2, 5, 6 and 7. The essential differences between the three types can best be pointed out in connection with the analysis of the data for each type in the later discussion. We may profitably turn, at this point, to the consideration of the second problem proposed at the beginning, *i.e.*, as to the relative difficulty of the three types of cul-de-sac above mentioned together with an interpretation of the same in terms of the reactive tendencies of the white rat.

That the first question as to the temporal order of elimination of culs-de-sac and the second one as to the relative difficulty of various types of culs-de-sac are practically identical is obvious. For it is a mere truism to say that an *easy* cul-de-sac will be eliminated *earlier* than a more difficult one or vice versa, provided, of course, the rate of effort put forth remains the same in both cases. In fact that is precisely what we mean, and all we can mean, by the use of the terms easy and difficult in connection with the process of learning. Ease and relative speed of elimination become identical terms under such conditions. It is equally apparent that the temporal order of elimination is a function of any and all factors which determine the relative difficulty of culs-de-sac.

An illustration of this principle is furnished by the distribution of errors among the various culs-de-sac of the maze, since this distribution offers an empirical basis for the ranking of the culs-de-sac in the order of their difficulty. At least the relative number of entrances may be taken as an index of the comparative difficulty of the various culs-de-sac as arranged in the specific pattern used, although it is quite possible that the same order of difficulty might not hold

under a different arrangement of the same set of culs-de-sac. The average number of entrances into the different culs-de-sac of the maze here used is indicated in the second line of Table II and it will be seen that a perfect ranking correlation exists between the total number of entrances and the temporal order of elimination. The fewer the number of entrances into a given cul-de-sac the earlier will it be eliminated.

TABLE II

RELATION BETWEEN TOTAL ENTRANCES INTO EACH CUL-DE-SAC AND NUMBER OF TRIALS REQUIRED TO ELIMINATE

Number of cul-de-sac.....	1	2	3	4	5	6	7	8	9
Ave. number trials required to eliminate.....	54.2	31.1	4.5	15.5	49.3	32.7	22.5	2.6	15.7
Ave. number entrances per animal.....	45.2	24.3	2.1	8.5	40.0	25.7	16.5	1.3	8.6
Rank of culs-de-sac in both cases.....	9	6	2	3	8	7	5	1	4

A similar relation in the case of a large number of mazes, and between various stages of the learning process has been found by Carr (1), who also points out the *a priori* rationality of such a correlation, inasmuch as it might naturally be expected that the strongest tendencies would be the hardest to overcome and, therefore, would require the longest time. Carr further states the principle, with which the results of the present experiment in general agree, that the order of elimination is in part a function of the strength of the entrance attraction of the various culs-de-sac, *i.e.*, a function of the *type* of cul-de-sac.

The application of this principle to the data of the present experiment yields several interesting relationships. Thus, a comparison of the values in Tables I and II with the three types of culs-de-sac indicated in Fig. I will show that those of similar type, although located at widely different parts of the maze, agree very closely with each other both as to the number of trials required to eliminate and as to the number of times entered. For example, numbers 8 and 3, comprising

type 1 as previously classified, have values very close together and were the first two culs-de-sac to be eliminated. Numbers 4 and 9 which form type 2 have practically tied values and, although next in order of elimination, are much higher in absolute terms than either of those of type 1 and, at the same time, decidedly lower than any of the remaining five culs-de-sac which together comprise type 3. It is clear from the above comparisons that the different types of cul-de-sac, regardless of their relative proximity to the foodbox, present essentially different different problems to the animal; that certain culs-de-sac are inherently less attractive than others, or perhaps better, that the avoidance of certain types is more in line with the natural reactive tendencies of the animal than the avoidance of others, and that the temporal order of elimination is in large part a function of the type of cul-de-sac.

TABLE III

RELATIVE DIFFICULTY OF THE THREE TYPES OF CULS-DE-SAC IN TERMS OF MEAN NUMBER OF ENTRANCES AND TRIALS REQUIRED TO ELIMINATE

	Type 1	Type 2	Type 3
Ave. number trials required to eliminate.....	3.6	15.6	38.0
Ave. number of entrances per animal.....	1.7	8.6	30.3

The relative difficulty of the three types of cul-de-sac included in the maze here employed is quantitatively indicated in Tables I, II and III. The question as to why the one type should be less difficult than another would require for solution a more complete knowledge of the reactive tendencies of the white rat than we at present possess. However, certain tentative explanations are suggested by a careful examination of the data in detail.

The evidence seems to be especially convincing, for example, in regard to the reason why the culs-de-sac of type I proved to be so easy in comparison with any of the others. These two culs-de-sac, numbers 8 and 3, were entered only about twice per animal, on the average, during the whole period of learning, whereas those of type 2 were entered about

eight and one half times, while those of type 3 were entered over thirty times as will be seen by referring to Table IV. Number 8 was not entered a single time by 11, while number 3 was not entered a single time by 6 of the 35 animals. Of even greater significance, however, is the fact that neither of the culs-de-sac was entered after the seventeenth trial by a single animal of the group, while only in 6 cases was either entered later than the tenth trial. Furthermore, 29 of the 35 rats eliminated both numbers 8 and 3 before any other cul-de-sac of the series. Only a few entrances occurred, therefore, and these invariably during the first few trials.

These two culs-de-sac are not different in form from the other seven, but it will be seen by referring to Fig. 1 that the cul-de-sac opening is so placed with respect to the true pathway that it lies entirely out of the line of direction when the animal maintains the forward orientation. All these facts taken together suggest a plausible hypothesis to account for the relative ease and speed of elimination of the type 1 cul-de-sac, viz., it is eliminated along with, and in much the same manner as return cul-de-sac errors by virtue of the early establishment of the habit of persistently maintaining the forward orientation, and without the necessity of building up a definite avoiding reaction pattern. The first few trials in maze learning may be called the period of initial adjustment which is characterized by frequent retracing, due largely perhaps to fear and curiosity. As soon, however, as the hunger stimulus becomes dominant over these emotional tendencies the animal no longer retraces but, keeping the forward orientation, goes directly and speedily to the foodbox. Both directness and speed would tend to favor the elimination of the type 1 cul-de-sac. It would seem to be significant that the period during which practically all entrances into this type of cul-de-sac occurred coincides approximately with the period of initial adjustment.

An inspection of Table II will show that culs-de-sac 4 and 9 comprising type 2 were entered only about one half as many times as the lowest of the five culs-de-sac of type 3. Either of the two culs-de-sac was entered after the twentieth trial

by only 7 of the 35 animals, although the average number of trials required to learn the maze as a whole was 70.2 for the group. Evidently the type 2 cul-de-sac is eliminated at a comparatively early stage of the learning process and, therefore, the real problem for the animal in a maze of this pattern is the elimination of those of type 3. A reference to Fig. 1 will show that a double turn is required in approaching and passing these two culs-de-sac, and that the opening lies to the side rather than directly in the line of direction. The uniqueness of the turn as compared to all others in the maze may have served as a cue and favored early elimination. The animal usually slowed down and appeared more cautious at these two turns instead of rushing pell mell into the opening, which was the characteristic behavior in the case of the type 3 cul-de-sac. The greater complexity of the turn may, therefore, have been actually a favorable factor in inducing a more successful type of reaction. The certain fact is that the avoidance reaction pattern required at a cul-de-sac of this type is speedily learned, whatever may be the causal factors involved.

The five remaining culs-de-sac included under type 3 are so placed with reference to the true pathway as to require identical types of motor coördination in the eliminative process. In each case, as can be seen by referring to Fig. 1, the general direction of the turn either into the cul-de-sac or along the true pathway is the same. The problem for the animal would seem to be, therefore, not a choice of directions, but the formation of a long *vs.* a short turning pattern, in which the largest factor would be a distance adjustment. The animal tends to turn too short, or too quickly, and thereby rushes headlong into the cul-de-sac, especially after it has developed a high running speed. Culs-de-sac of this type are seldom eliminated suddenly, the characteristic reaction being the gradual shortening of the distance traversed within the cul-de-sac until finally there is merely a false movement of the head as if to enter. This contrasts sharply with the behavior at types 1 and 2, since in both the latter cases sudden elimination was the rule. The final error was

made at a cul-de-sac of type 3 by every animal of the group, while the average number of entrances was three and one half times as great as for those of type 2, and over fifteen times as great as for those of type 1. The type of motor coördination required in making the adjustment for a turn of this character is evidently very difficult.

There is, however, a wider range among the values for these five culs-de-sac than would be expected if type of cul-de-sac were the only factor determining their relative difficulty, and hence their order of elimination. All are precisely alike spatially except that numbers 6 and 7 are 20 per cent. shorter than the other three, which fact may account, in part at least, for the comparatively low values of these two, inasmuch as Peterson has found short culs-de-sac easier to eliminate than longer ones. Numbers 1, 5 and 6 are so located as to require a right turn and have values consistently higher than numbers 2 and 7 which require a left turn, but whether this indicates a real difference due to the mere direction of the turn cannot be certainly known from the present data because of the presence of other factors. The wide difference in absolute values among these five highly similar culs-de-sac suggests that their relative difficulty is probably in part determined by the character of the preceding culs-de-sac in each case, the difficulty of a cul-de-sac being in part a function of its serial position—not indeed, with reference to the foodbox, but with respect to its relation to other types of cul-de-sac included in the same maze pattern. The measurement of this factor would, of course, require the comparison of certain standardized types of culs-de-sac under a wide variety of groupings.

The larger number of cul-de-sac errors occasioned by returns were made in the section of the maze nearest the entrance which accounts in part for the fact that the first cul-de-sac proved to be the most difficult. In fact, the number of entrances made into number 1 and number 5 do not differ greatly when allowance is made for the entrances due to retracing. However, the effect of retracing in increasing the number of cul-de-sac errors, even in the section

of the maze nearest to the entrance, is significantly small as the absolute values of Table III clearly indicate. This is what might be expected in view of the fact that retracing is practically confined to the first few trials of learning. The distribution of this type of error is, therefore, of small consequence in determining the final order of elimination, although its importance has been urged by several investigators in this field. Clearly it is a factor of doubtful value in any explanation of regressive elimination of culs-de-sac where such may be found.

TABLE IV

EFFECT OF RETRACING IN INCREASING THE NUMBER OF FORWARD CUL-DE-SAC ENTRANCES

Number of cul-de-sac.....	1	2	3	4	5	6	7	8	9
Total forward errors.....	45.2	24.3	2.1	8.5	40.0	25.7	16.5	1.3	8.6
Forward errors due to retracing.....	2.4	1.0	.14	.34	.2	.23	.06	0	0

If relative proximity to the foodbox were a condition of any consequence in determining the order of cul-de-sac elimination, then there ought to be some tendency to the regressive order in the case of the five highly comparable culs-de-sac of type 3. But such is not the case as will appear by a comparison of the values for numbers 1, 2, 5, 6 and 7 as given in Tables I and II. The order of elimination for these five taken alone is 7, 2, 6, 5, 1, with the values for the first three lying relatively close together, while those for numbers 1 and 5 are also nearly equal but much higher than the first three. While a crucial test could be made, perhaps, only in a maze involving but a single type of cul-de-sac, still the evidence seems rather convincing that relative proximity to the foodbox has little or nothing to do with the order of elimination, inasmuch as its influence is not apparent even in a series of highly similar culs-de-sac.

There is no theoretical ground, so far as the writer can see, for the supposition that, in the mastery of a complex habit of the maze type, a unit coördination nearer the food would

be formed more quickly or easily than one of identical type located only a few inches, or at most a few feet farther away from the food, inasmuch as exactly the same kinæsthetic adjustment should be required in the two cases. The sole function of the food in animal learning, so far as is at present known, is to induce *general* and persistent activity on the part of the animal, whereas the essential problem of trial and error learning is to account for *selective* activity, the latter taking the form of definite reaction patterns in maze learning. The former can in no sense explain the latter. It is difficult to see how proximity to food *per se* could operate as a selective agency in enabling the organism to hit upon and fixate the successful muscular movements more easily or speedily in a given cul-de-sac situation.

The point may, of course, be urged that as the animal approaches the foodbox the intensity of the hunger stimulus is increased and this might serve to heighten the qualitative character of the reactions near the goal. However, it may well be doubted whether or not a consistent or significant variation of the intensity of the hunger stimulus at different points along the pathway actually occurs, in view of the few seconds usually required for a complete run, especially during the period of learning when the most rapid elimination of the more difficult culs-de-sac occurs. But even if this were established, it would not follow that the increase in intensity of the hunger stimulus would bring about a corresponding increase in the efficiency of the motor adjustment at points near the foodbox. The very opposite result might be produced instead, inasmuch as it is common knowledge that a too great intensity of the hunger stimulus induces uncoördinated movements and faulty adjustment. Any consistent increase in the intensity of the hunger stimulus as the animal approaches the foodbox would probably manifest itself chiefly in an increase in the speed of running, and there is no ground for assuming that a positive, rather than a negative correlation exists between speed of running and efficiency of motor coördination. It appears altogether unlikely, therefore, that a given cul-de-sac is made less difficult to eliminate by being located near to the food as the source of satisfaction.

However, upon purely objective grounds, culs-de-sac situated in the goal end of the maze do have a certain advantage over those located very near the entrance. This is due to the fact that the greater amount of retracing occurs near the entrance so that culs-de-sac in this position are likely to be entered more than once during a single trial. This factor proved to be of small consequence in the learning of the maze here employed, as has been previously pointed out in connection with the discussion of Table IV. Relatively few forward entrances into culs-de-sac are occasioned by retracings, even in the first section of the maze, for the simple reason that such retracing is practically limited to the first few trials, and the bulk of the errors are made after the period of initial adjustment is over. The advantage of culs-de-sac near the foodbox over those near the maze entrance, so far as the factor of retracing is concerned, is so slight that it can be practically ignored in dealing with the question of the temporal order of elimination of culs-de-sac.

It may also be pointed out in this connection that where a tendency to the regressive order of elimination of culs-de-sac may be found in a given case it is not at all necessary to assume that relative proximity to the foodbox was the determining, or even a contributing factor. Such an order might be a function of the arrangement of the different types of cul-de-sac with respect to each other, rather than their serial position with reference to the foodbox. If a preponderance of the more difficult types of cul-de-sac used in a given pattern should happen to be placed in the first section of the maze, this would obviously give the effect of regressive elimination, other things being equal.

It is not here contended that the order of elimination is altogether a function of the type of cul-de-sac. It is highly probable that the difficulty of a given type is in no small degree conditioned by that of others which precede it in the same pattern. For the problem for the animal in the mastery of a maze is, obviously, not merely the learning of this, that, and the other unit coördination represented by the different culs-de-sac as disparate acts, but the learning of these as

related to each other in a specific serial order. Moreover, the units are not first learned and afterward integrated into a total act, but rather the latter process takes place *pari passu* with the mastery of the unit coördinations involved. Whether there are still other factors, aside from type and interrelation of culs-de-sac, which are of importance in determining the order of their elimination is a matter to be determined by further experimentation.

Although the maze is especially adapted to the study of the complex motor habit analyzed into its more or less definite unit coördinations, the tendency among investigators has been to ignore the inner details of the process and to report maze data only *en masse*, and hence the rôle played in the total process by different types of unit coördinations is little understood. From the present study it is clear that culs-de-sac appearing highly similar to the human eye may represent almost incomparable differences in difficulty to the animal; that these differences can be explained in terms of the animal's reactive tendencies, and that the degree of difficulty of a maze is not proportional to the number of culs-de-sac, but is largely dependent upon the various types included. A more extensive and thoroughgoing analysis of the maze into typical cul-de-sac situations should finally lead to the quantitative ranking of these, reduced perhaps to a few essential types. This together with a like measurement of the factor of their interrelation in various patterns would offer an empirical basis for the construction of mazes of known difficulty and for the study of certain definite types of reaction. Moreover, it would undoubtedly yield a wealth of detail concerning the inner mechanics of the process of building up a complex motor habit.

Summary of Results

1. A perfect ranking correlation was found to exist between the relative number of entrances into the various culs-de-sac and the temporal order of elimination. The fewer the entrances made the earlier the elimination.

2. No apparent relation exists between the serial position

of a cul-de-sac with respect to the foodbox and the temporal order of elimination in the maze here employed. This held true when a series of five culs-de-sac of approximately identical type were alone considered.

3. The number of cul-de-sac entrances occasioned by retracing was very small and consequently their distribution is not significant in determining the order of elimination of culs-de-sac.

4. The temporal order of elimination is determined in large part by differences in the character of the motor coordination required in the avoidance reaction pattern of the different types of cul-de-sac.

5. The differences in difficulty between the three types of cul-de-sac included in the maze here used were extremely large, both in terms of the number of errors made and in terms of the number of trials required to eliminate. Type 3 (numbers 1, 2, 5, 6, 7) which involved an adjustment for a long *vs.* a short turn proved to be by far the most difficult. Type 2 (numbers 4, 9) which involved a complex turn, with the cul-de-sac lying to the side of the true pathway was the next in order. Type 1 (numbers 8, 3) in which the opening was located backward from the line of direction when the animal maintained the forward orientation offered comparatively slight difficulty.

6. Some preliminary suggestions have been made toward the interpretation of the differences in difficulty of the three types of cul-de-sac here investigated in terms of the reactive tendencies of the animal, as indicated in the quantitative data and in the observed behavior of the animals.

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BISECTION OF TONAL INTERVALS SMALLER THAN AN OCTAVE

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As an illustration of the logarithmic relation which seemed to obtain in various sensory dimensions between stimulus and mental process, the early psychophysicists were wont to cite the case of the musical scale in which similar intervals from different parts of the scale are paralleled on the side of vibration frequencies by numerical values which bear a constant ratio to one another. It was quite generally assumed that in the development of the musical scale art had performed a valid experiment for the science of psychophysics. "Inzwischen bedarf es zur Bestätigung des Gesetzes (Weber's) in dieser Hinsicht nicht erst besonderer Versuche, da es die einfache und sozusagen notorische Aussage des musikalischen Gehöres ist, dass gleichen Verhältnissen der Schwingungszahlen eine als gleichgross empfundene Tondifferenz in verschiedenen Oktaven entspricht, so dass man das Gesetz hier direkter als sonst irgendwo und zwar auch für grosse Unterschiede erwiesen kann."[1] It was not until thirty years[2] after Weber first pointed to the musical scale in support of his generalization regarding the relation between body and mind that psychologists began to question the validity of the assumption. Müller declared that "die Anordnungsweise unserer Tonleitern kann uns über das zwischen Schwingungszahl und Tonhöhe bestehende functionelle Verhältniss keine Auskunft geben, weil der Aufbau der

[1] G. Th. Fechner, 'Elemente der Psychophysik,' rep. 1888, I., 181. *Cf.* also E. H. Weber, 'Tastsinn und Gemeingefühl,' ed. by E. Hering, 1905, 116; H. Helmholtz, 'Physiologische Optik,' 1867, 312; W. Wundt, 'Physiologische Psychologie,' 1874, 363. For an historical and critical discussion, see E. B. Titchener, 'Experimental Psychology,' 1905, Vol. II., pt. ii, 232–248.

[2] In 1876 with the publication of W. Preyer's 'Die Unterschiedsempfindlichkeit für Tonhöhen' and P. Langer's 'Die Grundlagen der Psychophysik.'

Tonskalen nicht im Geringsten durch psychologische oder psychophysische Gesichtspunkte, sondern nur durch das Princip der Klangverwandschaft bestimmt worden ist." [1] Stumpf was inclined to agree with Müller's arguments and added in support of them certain factual evidence that the same interval does not represent the same tonal distance in all parts of the scale. "Intervall und blosser Unterschied (Distanz) ist zweierlei; und keineswegs leuchtet unmittelbar ein, dass das gleiche Intervall überall die gleiche Distanz darstelle." [2]

The first experimental attack on the problem was made by Lorenz in Wundt's laboratory.[3] By a combination of the methods of right and wrong cases and equal sense distances Lorenz sought to determine that value of auditory stimulus which lay for sensation midway between two limiting stimuli. From the results of these experiments Lorenz concluded that the perception of differences in pitch does not conform to Weber's law; that, on the contrary, the same harmonic intervals represent unequal differences of sensation, and that between the absolute differences of sensation and the corresponding differences in vibration frequencies there exists an almost exact proportionality.[4] These conclusions and their unqualified acceptance by Wundt constituted the *casus belli* in the heated controversy between Stumpf and Wundt as to whether the psychological mid-point of a given tonal distance falls on the arithmetical or geometrical mean.[5] While this controversy was in progress Münsterberg published an account of some experiments of his own on the comparison of tonal distances.[6] The results agree for the most part with those of Lorenz except for those intervals wider than a double octave.

The results of the present study cannot be expected to

[1] G. E. Müller, 'Zur Grundlegung der Psychophysik,' 1878, 284.

[2] C. Stumpf, 'Tonpsychologie,' 1883, I., 337.

[3] C. Lorenz, 'Untersuchungen über die Auffassung von Tondistanzen,' *Philos. Stud.*, 1891, 6, 26–103.

[4] *Ibid.*, 103.

[5] A list of references to this prolonged controversy is given by Titchener, *op. cit.*, 242.

[6] H. Münsterberg, 'Beiträge zur experimentellen Psychologie,' 1892, IV., 147–177.

bear directly on the controversial issue raised by Lorenz' investigation inasmuch as the experiments constitute only the initial approach to a problem of a different nature from the one with which Lorenz was primarily concerned.[1] For the sake of clarity, however, these preliminary quantitative determinations may perhaps best be presented in the setting which Lorenz' study gave to the problem of the bisection of tonal intervals. The experiments were performed in the Psychological Laboratory of Clark University in the spring of 1922.

The *O*s who took part in the experiment were H, Dr. H. M. Halverson; B, Dr. M. Bates; M, Mr. M. K. Macdonald; and T, Mr. M. A. Tinker. All were well trained in psychological observation. H and B were musical *O*s and had had considerable experience observing in studies in audition. M and T were unmusical *O*s to the extent of being unable to recognize harmonic intervals, although they had more or less "ear" for occidental melodic form.

The immediate object of these initial experiments was to determine for a given tonal interval that value of auditory stimulus which for a given *O* divided the interval into two equal parts. The experimental procedure adopted followed the usual form of the so-called method of equal sense distances with such alterations as were necessary to secure data which could be treated by Urban's phi-gamma process of calculation.[2] Two limiting stimuli, constituting the components of the tonal interval, were kept constant throughout the procedure while a series of intermediate stimuli at the central region of the interval served to separate the interval into two sense distances. These two sense distances were then the

[1] It is hoped to continue the investigation to bisections of tonal sense distances wider than the octave, and eventually to a qualitative analysis of the sense distance itself and the judgment of equality.

[2] See Titchener, *op. cit.*, Vol. II., pt. I., 81–92, for a description of the method of equal sense distances. The distinction between *method* and *process* in psychophysical work is made by Brown and Thomson, 'The Essentials of Mental Measurement,' 1921, 46f. The rules for the phi-gamma process of calculation are given by F. M. Urban, 'Hilfstabellen für die Konstanzmethode,' *Arch. f. d. ges. Psychol.*, 1912, 24, 236–243. *Cf.* in this connection E. B. Holt, 'The Classification of Psychophysic Methods,' *Psych. Rev.*, 1904, 11, 343–369.

objects of judgment for *O*. Given, *e.g.*, the stimuli *l* (lower component of the interval), *m* (middle stimulus), and *h* (higher component of interval), in the order named, it was the task of *O* to judge the distance *mh* with respect to the distance *lm* in terms of the categories 'greater,' 'equal' and 'less.' To avoid the time error the stimuli were also presented in the order *h*, *m*, *l*, in which cases *O* judged the distance *ml* with respect to the distance *hm*. The duration of every stimulus was one second and the separation between stimuli was also one second, measured by a silent metronome. Half of the presentations during an hour's observation were in the ascending order, and half in the descending order.

The instructions to *O* read:

"Following the 'ready' signal you will be presented with three successive tones. You are to compare the distance between the second and third tonal sensations with respect to the distance between the first and second tonal sensations. Express your judgment by one of the three categories 'greater,' 'equal,' and 'less.' A judgment 'greater' or 'less' indicates that the second auditory sense distance is greater or less than the first; the judgment 'equal' means that the second sense distance is equal to he first. Occasionally you will be asked to give either a generalized introspective account of the nature of your judgments on sense distances, or a complete introspection of a given particular judgment."

The judgments which these instructions called for were not easy for the *O*s to make at first. It was found necessary with every *O* to devote considerable time to preliminary experimentation in order that such a series of *m* stimuli could be chosen and kept constant as would yield relative frequencies of judgments of 'greater,' 'equal' and 'less' upon the two sense distances in accordance with the demands made by Urban's phi-gamma process of calculation. Every *O* experienced more or less initial difficulty in getting oriented to the task. And it happened in many cases that an *O* had to reorient himself to the second time order of presentation after he had gotten one orientation to the first time order. This curious shift in attitude between ascending and descending orders made it necessary to use different sets of *m* stimuli with certain *O*s for the two orders, as can be seen by a comparison of the stimulus values in Table IV. After the initial trials, however, all the *O*s settled down to their *Aufgabe* without much further difficulty, and gave judgments whose

relative frequencies showed not a single inversion after the 'equal' judgments had been halved between the 'greater' and 'less' judgments.[1] It was therefore possible to determine by calculation on the basis of Urban's phi-gamma hypothesis that ideal value of *m* stimulus which in 50 per cent. of the cases would divide the interval into two psychologically equal parts. All the relative frequencies for calculation of these medians were based on fifty judgments on every pair of intervals in the ascending order, and fifty judgments on every pair in the reverse order—a total of 7,100 judgments for the whole experiment.

In Series I the limiting stimuli (*l* and *h*) were 300 d.v. and 410 d.v.: an amusical interval slightly larger than the natural fourth. The *m* stimuli ranged between 340 and 360 d.v. by equal steps of 2.5 d.v. The same *m* stimuli or the same number of *m* stimuli could not be used, however, for every *O*. In order to secure psychometric functions of ogive form it was necessary, *e.g.*, to use seven stimuli (340–355 d.v.) in the descending order with T and only five stimuli (342.5–352.5 d.v.) in the descending order with B. (See Table IV.)

The geometrical mean for the interval in Series I is 350.71, and the arithmetical mean, 355. In Table I will be found

TABLE I

SERIES I

Psychophysical bisections of the interval 300–410 d.v. for four *O*s. Abbreviations: a = ascending order, d = descending order, av = average of ascending and descending orders, Dg = difference between average median and geometrical mean, Da = difference between average median and arithmetical mean, P.E.av = probable error of the average median, P_s = probability that average median will not be nearer the arithmetical than the geometrical mean.

$l = 300$, geometrical mean = 350.71,
$h = 410$, arithmetical mean = 355.

*O*s	a	d	av	Dg	Da	P.E.av	P_s
H.....	349.40	346.68	348.04	2.67	6.96	3.72	0.8078
B.....	349.00	347.58	348.29	2.42	6.71	2.27	0.9115
M.....	344.48	349.18	346.83	3.88	8.17	4.76	0.8023
T.....	355.82	346.93	351.37	−0.66	3.63	5.58	0.5696

[1] For an account of this process of calculation in the method of equal sense distances, see Titchener, *op. cit.*, 85f.

the medians for all four *O*s in the ascending and descending orders, the average of these ascending and descending medians, the differences between this average and the geometrical and arithmetical means, the P.E. of the average (which is at the same time the P.E. of the differences between the average and the two means), and the probability that the average median will not be nearer the arithmetical than the geometrical mean. It will be noted that for every *O* the difference between the average median and the geometrical mean is less than that between the average median and the arithmetical mean. In other words, the equal-appearing intervals are paralleled by *relatively* equal differences of stimulus.

The limiting stimuli in Series II were 285 d.v. and 427.5 d.v.: the musical interval of the natural fifth. The *m* stimuli between *l* and *h* ranged from 330 to 360 d.v. by equal steps of 2.5 d.v. or, in the case of one *O*, 5 d.v. It was found that with T 2.5 d.v. was too small a unit to give appreciable differences along the scale of *m* stimuli so that the steps had to be widened to units of 5 d.v.

The geometrical mean for the interval of the fifth used in this series is 349.05, and the arithmetical mean, 356.25. From Table II it can be seen that the results of Series II are quite similar to those of Series I. For every *O* the median of the interval lies nearer the geometrical than the arithmetical mean. The psychologically equal divisions of the interval, therefore, correspond to relatively equal differences in vibrations.

Since the tones 285 and 427.5 constitute a musical interval it may be interesting to note the relation of the psychophysical divisions of the interval to the divisions made use of in music. For a fifth with a tonal root of 285 d.v. the minor third equals 342 d.v. and the major third equals 356.25 d.v.; and the tempered minor and major thirds equal 338.9 d.v. and 359.05 d.v. respectively. It so happens that none of the ascending and descending medians or average medians corresponds to any one of these musical divisions. All of the medians, in fact, lie somewhere *between* the natural and tempered minor thirds and the natural and tempered major

thirds, and, it is worth noting, somewhat nearer the natural than the tempered intervals. But perhaps this closer proximity to the natural intervals is due to the fact that in modern equal temperament the natural minor third is lowered and the natural major third is raised, thus setting the tempered intervals farther removed than the natural thirds in both directions from the geometrical mean of the fifth. For since, as has been noted, the psychophysical medians fall in the vicinity of the geometrical mean, they are *ipso facto* farther from the tempered than from the natural intervals of music.

TABLE II

SERIES II

Bisections of the Interval 285–427.5 d.v.
l = 285, geometrical mean = 349.05,
h = 427.5, arithmetical mean = 356.25.

*O*s	a	d	av	Dg	Da	P.E.av	P_s
H.....	347.25	347.65	347.45	1.60	8.80	2.41	0.9265
B.....	354.98	347.76	351.37	−2.32	4.88	2.40	0.6396
M.....	345.02	351.07	348.04	1.01	8.21	4.03	0.7790
T.....	350.96	346.40	348.68	0.37	7.57	7.77	0.6345

For Series III the limiting stimuli used were 285 d.v. and 510 d.v.: an amusical interval slightly larger than a tempered minor seventh. Orientation to this series of experiments seemed to meet with greater difficulty and require longer practice than the two preceding series. The *m* stimuli ranged from 355 to 420 d.v. For two *O*s equal separations of 2.5 d.v. yielded the required distribution of judgments, while for the other two *O*s it was necessary to use separations of 5 d.v. The lowest *m* stimulus in the ascending order for T was ten vibrations higher than the highest *m* stimulus in the descending order; and for B the lowest *m* stimulus of the ascending order was the same as the highest of the descending order. Such were the necessary variations in the choice of stimuli which served to bisect the interval into the parts upon which psychophysical judgments were passed.

The geometrical mean for the interval in Series III is 381.25, and the arithmetical mean, 397.5. A glance at Table

III reveals the fact that here again the average medians are much nearer the geometrical than the arithmetical mean except for B. For B the median of the descending order is nearer the geometrical mean, but the median of the ascending order coincides almost exactly with the arithmetical mean, and the average of these two medians is 5.13 d.v. nearer the arithmetical than the geometrical mean. The reason for this divergence from the geometrical mean is not apparent, at least from the introspective reports. An explanation would have to be sought in quantitative analyses of the qualitative factors enumerated in the introspective descriptions. It hardly seems likely that the divergence is due to B's musical training, for a musical criterion of judgment would tend to bring the division near the musical (= geometrical) mid-point of the interval. B stated, moreover, in her introspective reports that musical knowledge did not cut across her psychological criteria of judgment. The average psychophysical medians for the other *O*s, however, coincide very closely with the geometrical mean, thus dividing the interval into two equal sense distances which are paralleled by relatively equal differences in vibration frequencies.

TABLE III

SERIES III

Bisections of the Interval 285–510 d.v.
l = 285, geometrical mean = 381.25,
h = 510, arithmetical mean = 397.50.

*O*s	a	d	av	Dg	Da	P.E.av	P_z
H.....	383.39	377.36	380.37	0.88	17.13	3.31	0.9667
B.....	397.49	386.40	391.94	−10.69	5.56	3.46	0.3089
M.....	374.11	379.65	376.88	4.37	20.62	7.32	0.8742
T.....	401.32	364.41	382.86	− 1.61	14.64	7.26	0.7258

The mathematical significance of the average medians with respect to their proximity to the geometrical means is shown by the values of P_z in the last columns of Tables I, II and III. Since that value which lies half way between the geometrical and arithmetical means delimits the range of distributions on the curve of probability which the average

median cannot exceed without being nearer the arithmetical than the geometrical mean, the significance of the difference between the average median and that value, or, in other words, the probability that the average median will not be nearer the arithmetical than the geometrical mean, has been calculated from the P.E. of the average median on the basis of the ratio z as an argument to the error function of the probability integral.[1] It will be noted that all the values of P_z, with the exception of B in Series III, are greater than 0.5. Most of the values are considerably greater than 0.5, some, in fact, reaching almost mathematical certainty. The low value of B in Series III was to be expected since, as has been noted above, her average median in that case was nearer the arithmetical than the geometrical mean.

The differences in the positions of the psychophysical medians due to the time error of the ascending and descending orders are not sufficiently uniform to permit of generalization. It is perhaps significant, however, that for every *O*, with one single exception for H, the differences between the ascending and descending orders are always in the same direction. For M the ascending medians for every series of experiments are always lower than the descending; for B and T the ascending medians are always higher; and for H the ascending medians are also higher except in Series II in which case the ascending and descending medians are practically the same: 347.25 and 347.65. Two thirds (8 out of 12) of the medians of the ascending order are higher than those of the descending order.

It seems, then, that the results of these experiments tend quite uniformly to place the mid-point of tonal intervals smaller than an octave at the geometrical and not at the arithmetical mean. A logarithmic relation does seem to obtain between the stimuli and the concomitant mental processes, although it is not possible to state just what the nature of this relationship is, inasmuch as the intermediate factors between stimulus and report upon process are not

[1] W. W. Johnson, 'The Theory of Errors and the Method of Least Squares,' 1915, 36.

sufficiently well known. Not until these intermediate links are isolated and examined in their own right will it be possible to understand the relationship between body and mind, or even to state Weber's law in anything like intelligible language. If the judgments with which one works in psychophysical experimentation are not univocally determined by a specific mental process, the statistical correlations, no matter how refined, will be equivocal and, at best, interesting only as behavioristic responses.[1]

It was with the purpose in mind of securing qualitative data which could be used subsequently as a means for securing finer control of the total chain of events between stimulus and report that introspections were asked for from the *O*s. It may not be out of place to remark in this connection that the introspections give abundant evidence of the secondary criteria of judgment to which the method of equal sense distances has always been considered peculiarly liable.[2] Visual schemata, especially, exercise a strong and insidious coercive power over judgment,[3] as the following introspective excerpts illustrate:

Ob. H. "The auditory perceptions are frequently accompanied by unclear visual imagery. The three tones form a kind of balancing bar placed horizontally before my eyes: the ends of the bar are fairly plain and represent the first and last tones. Near the middle of the bar is a very vague object representative of the middle tone. It is spherical and fuzzy and of very thin texture."

Ob. B. "Visual image, less clear than auditory process, corresponding to pitch of tones consisted of two vertical lines representing first and third tones. With the third tone the visual schema was complete with the appearance of a third vertical line somewhere between the other two."

Ob. M. "A visual diagram enters also, for as I hear the tones they place themselves in a line, the first and last always being in the same place, and the middle or second moving up or down according to its pitch."

Ob. T. "First tone projected straight ahead of me and placed on a neutral background as a spot. The second tone was immediately projected downward at a sharp

[1] See E. G. Boring, 'The Control of Attitude in Psychophysical Experiments,' *Psychol. Rev.*, 1920, 27, 440–452; 'The Stimulus Error,' *Amer. J. Psychol.*, 449–471, esp. 465 ff.

[2] G. E. Müller, 'Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik,' 1904, 237 f.; Titchener, *op. cit.*, Vol. II., Pt. II., 203 f.

[3] The prevalence of visual schemata in work on sound has often been noted. *Cf*, *e.g.*, F. Angell, 'Untersuchungen über die Schätzung von Schallintensitäten,' *Philos. Stud.*, 1892, 7, 438 f; C. C. Pratt, 'Some Qualitative Aspects of Bitonal Complexes,' *Amer. J. Psychol.*, 1921, 32, 505.

TABLE IV

RELATIVE FREQUENCIES OF JUDGMENTS ON TONAL SENSE DISTANCES AFTER 'EQUAL' JUDGMENTS HAVE BEEN HALVED BETWEEN 'GREATER' AND 'LESS' JUDGMENTS

The numbers in the first column refer to the values of *m* stimuli used in the three series; the italicized numbers refer to the limiting stimuli (*l* and *h*) of the series; a = ascending order, d = descending order.

Os.....	H		B		M		T	
	a	d	a	d	a	d	a	d
Series I								
410								
360	.....	.....	.....	.....	.....	.....	0.82	.....
357·5	0.89	.....	.....	.....	.....	0.86	0.70	.....
355	0.80	0.87	0.90	.....	.....	0.72	0.43	0.81
352·5	0.62	0.83	0.67	0.93	0.84	0.45	0.36	0.73
350	0.42	0.63	0.32	0.84	0.75	0.40	0.35	0.59
347·5	0.27	0.45	0.15	0.40	0.69	0.33	0.23	0.59
345	0.18	0.24	0.07	0.22	0.58	0.29	.....	0.55
342·5	0.10	0.13	.....	0.07	0.27	0.15	.....	0.20
340	.....	.....	.....	.....	0.12	.....	.....	0.19
300								
Series II								
427·5								
360	.....	.....	0.94	.....	.....	.....	0.82	0.90
357·5	.....	.....	0.73	.....	.....	0.83	.....	.....
355	.....	.....	0.54	.....	.....	0.74	0.68	0.81
352·5	0.84	0.90	0.20	0.88	.....	0.60	.....	.....
350	0.76	0.81	0.08	0.76	0.80	0.46	0.36	0.58
347·5	0.48	0.52	.....	0.47	0.72	0.27	.....	.....
345	0.20	0.19	.....	0.23	0.55	0.16	0.28	0.52
342·5	0.08	0.04	.....	0.07	0.32	.....	.....	.....
340	.....	.....	.....	.....	0.20	.....	0.15	0.24
337·5	.....	.....	.....	.....	.....	.....	.....	.....
335	.....	.....	.....	.....	.....	.....	0.12	0.14
332·5	.....	.....	.....	.....	.....	.....	.....	.....
330	.....	.....	.....	.....	.....	.....	0.08	.....
285								
Series III								
510								
420	.....	.....	.....	.....	.....	.....	0.86	.....
415	.....	.....	.....	.....	.....	.....	0.81	.....
410	.....	.....	.....	.....	.....	.....	0.63	.....
405	.....	.....	.....	.....	.....	.....	0.17	.....
402·5	.....	.....	0.91	.....	.....	.....	.....	.....
400	.....	.....	0.70	.....	.....	.....	0.14	.....
397·5	.....	.....	0.54	.....	.....	.....	.....	.....
395	.....	.....	0.18	.....	.....	0.96	0.11	.....
392·5	.....	.....	0.09	0.88	.....	.....	.....	.....
390	.....	.....	.....	0.70	0.87	0.91	0.05	.....
387·5	0.88	.....	.....	0.61	.....	.....	.....	.....
385	0.77	.....	.....	0.27	0.77	0.62	.....	.....
382·5	0.46	0.89	.....	0.25	.....	.....	.....	.....
380	0.40	0.76	.....	0.22	0.66	0.48	.....	0.88
377·5	0.20	0.49	.....	.....	.....	.....	.....	.....
375	0.16	0.29	.....	.....	0.49	0.34	.....	0.80
372·5	.....	0.12	.....	.....	.....	.....	.....	.....
370	.....	.....	.....	.....	0.33	0.17	.....	0.76
365	.....	.....	.....	.....	0.14	0.08	.....	0.60
360	.....	.....	.....	.....	0.09	.....	.....	0.23
355	.....	.....	.....	.....	.....	.....	.....	0.23
285								

angle and placed on the neutral background directly under the first tone. The third tone was projected still lower and placed on a background directly below 1 and 2, but only a short distance below 2 as compared to the distance between 1 and 2."

Kinæsthetic processes and integrated visual-kinæsthetic schemata are also mentioned by some *O*s. A not uncommon type of judgment is the kind based on a preconceived idea in *O*'s own mind of the position which the mid-point of the interval should occupy. This absolute judgment is formulated after the perception of the second tone.

Conclusions.

1. It is possible by a use of the method of equal sense distances with such alterations as are necessitated by the application of Urban's phi-gamma process of calculation to secure judgments on relative size of adjacent tonal sense distances which follow a continuous function of ogive form.

2. Under the conditions of this experiment the point of bisection of intervals smaller than an octave falls nearer the geometrical than the arithmetical mean. This result is in direct contradiction to those obtained by Lorenz.

3. The total perceptual pattern upon which judgments of tonal sense distances are made is complex and too ambiguous to permit of any interpretation of the logarithmic relation which seems to hold between stimulus and mental process.

ACCOMMODATION AND CONVERGENCE UNDER LOW ILLUMINATION[1]

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Statement of Problem

The principal purpose of this investigation was to compare, at low intensities of illumination, the degree of accuracy in accommodation and convergence, in the discrimination of relative distances. The self-adjustment method (*Selbsteinstellungsmethode*) was used, with small white beads as stimulus objects. One of the beads, the standard, was set each time at a distance of 75 cm. from the eye, and the subject moved the other bead until it and the standard appeared equally distant from him.

Previous Work

While the literature on vision is very extensive for nearly all topics, it is not possible to find much concerning actual experimental work in which accommodation and convergence are differentiated.

Wundt (10) used an apparatus in which the subject viewed, through horizontal slits, a taut vertical thread against a plain white background. The experiment was made with a view toward determining to what degree of certainty the relative distance of the thread could be determined when it was made to approach or recede. The subject was told to close his eyes each time before the thread was moved, and upon reopening them, he looked first at the screen and then at the thread, announcing his decision as to whether the thread had been made to approach or to recede. From the results it was concluded that the approach or recession of the thread could be detected whenever the motion was through a dis-

[1] From the Psychological Laboratory of Ohio State University. Supervised by Professor A. P. Weiss.

tance equal to at least 1/50 of the standard distance. Thus at a distance of 50 cm. an approach or recession of 1 cm. was regularly detected. There was no standard present with which the subject could compare the distance of the variable; the decisions were made on the basis of the previous setting of the thread just before the eyes were closed. The intensity of illumination used is not reported. Wundt used the results merely as proof that accommodation and convergence are effective and fairly accurate criteria in the discrimination of relative distances.

Bourdon (3) performed a series of experiments very similar to those of Wundt, but in which he used as stimulus a 'very small object' and made his measurements in terms of angles of the visual axes. He found that at a distance of 1.08 meters, a divergence or convergence of 8 minutes of arc for each eye could be detected. That is, at the given distance a convergence or divergence of 16 minutes or about a quarter of a degree could be regularly detected. These results, when translated into the same terms, agree very well with those of Wundt.

A considerable amount of investigation has been carried on in the field of visual acuity (defined as the ability to distinguish detail). Different light intensities and colors have been used, chiefly with the aim toward determining the most effective conditions of artificial lighting. The test objects used have been so varied and conditions so different that few uniform conclusions have been reached. It seems fairly well established, however, that, due to chromatic aberration, white light is less effective for detail discrimination than monochromatic light. The work of Luckiesch (5), Bell (1), and Rice (7) has been extensive in this field. There has been some investigation of visual reaction time for the recognition of test objects under different illuminations, notably that done recently by Elliott (4) from which the conclusion is drawn that the minimum intensity of illumination required for maximum speed of discrimination of details is between ten and twenty foot-candles. In the field of visual sensitometry (threshold determination) there have been investigations by Nutting (6), Blanchard (2), and others.

Apparatus and Method

The apparatus used in this experiment was somewhat similar to that used by Wundt and Bourdon. The subject viewed two small white beads through narrow horizontal slits and made adjustments of one of the beads by turning a knob at the side of the hood. The subject tried to set the adjustable bead so that it and the standard would be equally distant from him. At the end of each adjustment the experimenter read off the error (longitudinal distance between the adjusted bead and the standard) from a millimeter scale on the top of the apparatus. All shadows and reflections were eliminated by an enclosing boxlike hood covering the whole apparatus and painted dull black on the inside.

The stimulus beads were white and about 6 mm. in diameter. They were suspended from the top of the apparatus at the level of the subject's eyes by stiff, blackened wires, fine enough to be almost invisible at the low intensities of illumination used. At the top, the wires were mounted in travelling slides, which could be moved either by the turning of the subject's knobs or by the direct handling of the experimenter. The slides carried the scales and indicators for the reading of errors. The standard bead was set at the distance of 75 cm. for all observations. In order to eliminate the factor of right- or left-handedness, the left bead was used as the standard throughout the series so that all adjustments were made with the right hand. The lateral distance between the beads was 15 cm. so that it was necessary to fixate each separately. To eliminate the possibilities of the factors of relative size and brightness entering as criteria, one bead was made slightly smaller than the other and the gloss removed from its surface. The attention of each subject was called to these differences in the beads, and it was demonstrated to him that judgments could not be made on the basis of either brightness or size. Two shutters allowed the experimenter to investigate either the right or left eye separately or both eyes together.

The error readings were made in both directions from the perfect or zero position. The total range of the apparatus

was about 100 mm. in each direction; in any case where the bead was moved to the limit and effort was made to move it farther, the adjustment was called a failure and assigned the error value of 120 mm. During the time the error was being recorded, the subject removed his hand from the adjusting knob and sat with closed eyes. Before every setting the experimenter moved the right bead out of alignment while the subject's eyes remained closed. Whenever the subject had moved the bead around until he was satisfied that it was at the same distance from him as the standard, he called out 'ready', and the error was recorded by the experimenter.

It is impossible, of course, to isolate either accommodation or convergence while both eyes are allowed to function, but by shielding one eye convergence is automatically eliminated while accommodation remains in function. Adjustments were first made with both eyes, to give an indication of the effectiveness of accommodation and convergence together. Adjustments were then made with one eye screened, to give an indication of the effectiveness of accommodation alone.

The source of illumination was a 75-watt Daylight Mazda lamp. Four thicknesses of yellow gelatine were placed before the lamp to give a more uniform color to the light over the whole range of intensities. Four intensities of illumination were used. The highest corresponded to low intensity of ordinary artificial lighting conditions and was bright enough so that the discriminations were made with ease. There were two intermediate steps between this high intensity and the lowest, at which the observer was barely able to see the beads in indirect vision. The intensities of illumination, as measured at the standard distance with the Macbeth Illuminometer, are given in Table I.

TABLE I

ILLUMINATION INTENSITIES AT 75 CM. IN FOOT-CANDLE POWER

Intensity 1	Intensity 2	Intensity 3	Intensity 4
.0005	.0008	.0018	.0117

The values as given represent the averages of a number of

readings with the instrument. The reliability, however, is not nearly so great as the very small fractions imply. It was very difficult to get any measure at all for such low illuminations.

An experimental series for one subject consisted of five adjustments each for (*a*) both eyes, (*b*) right eye, (*c*) left eye, at each of the four illuminations. The series began with the highest illumination, *4*. This was followed by *3*, *2*, and *1*, with a final return to *4* for a repetition. The repetition of the adjustments at the first level was to neutralize practice effects. The results given for the highest illumination, *4*, are thus the averages of the initial and final adjustments. A series for one day thus consisted of 75 adjustments. Three such series were run on three different days for each of six subjects, with the time between the first and last sitting about three weeks in each case.

The subjects were all experienced in psychological observation. Three (*We*, *Ax*, *Fa*) were instructors in psychology; two others (*Wi*, *Ne*) were seniors taking experimental psychology; the sixth (*Du*) was a senior who had some laboratory training. In the case of one (*Ax*) there was a rather great degree of astigmatism along with some oculomotor trouble; this is plainly evident in some of his results.

Results

The values given in Table II and Table III and graphically represented in Fig. 1 show that as illumination is decreased a point is reached (illumination *1*) where accommodation alone ceases to function within practical limits of accuracy, the average error being 12 per cent. of the standard distance. At this same intensity with convergence functioning, distance discriminations were made with an average error of only 4 per cent. of the standard distance. The rate of increase in error is almost the same for both accommodation alone and with convergence as intensity is reduced until the level of illumination *2* is reached when the error for accommodation alone begins to increase very sharply. At the lowest

illumination, *1*, the average error for accommodation alone is three times that for accommodation plus convergence.[1]

1. *Comparison of Accommodation Alone and With Convergence at Different Low Intensities*

TABLE II

AVERAGE ERROR IN MILLIMETERS FOR EACH SERIES[2]

Subj.	Both Eyes (Convergence)				Right Eye (Accommodation)				Left Eye (Accommodation)			
	I–4	I–3	I–2	I–1	I–4	I–3	I–2	I–1	I–4	I–3	I–2	I–1
Ax.....	32.2	59.4	70.0	51.3	27.3	31.5	25.0	20.3	53.5	99.0	*F*	*F*
	53.6	48.2	67.0	96.2	24.9	35.2	45.1	*F*	50.3	25.8	42.8	*F*
	11.9	40.4	86.2	55.6	30.8	49.6	78.2	*F*	22.0	48.2	33.0	76.4
Du.....	22.1	56.4	78.9	49.6	23.2	98.6	32.3	*F*	19.2	51.6	51.8	*F*
	4.0	6.4	14.4	29.6	23.2	27.2	69.0	43.7	24.1	29.4	31.8	80.1
	14.7	11.2	15.8	31.0	33.1	50.2	43.9	69.0	31.6	34.0	63.4	49.1
Fa.....	10.7	31.6	22.0	20.8	39.8	73.0	34.0	40.2	28.5	25.2	31.5	69.0
	7.9	14.4	28.8	43.8	31.6	37.6	33.8	77.6	33.9	57.0	31.6	62.8
	5.1	13.6	18.6	58.0	28.5	79.2	81.2	71.2	30.2	67.4	83.6	*F*
Ne.....	8.4	12.4	26.8	21.0	64.4	35.6	86.2	*F*	58.7	37.4	84.6	*F*
	19.0	3.2	8.0	10.0	63.3	46.4	42.2	*F*	58.4	37.4	62.2	*F*
	5.8	6.6	13.2	27.4	52.1	24.4	64.0	*F*	31.6	44.8	*F*	*F*
We.....	6.8	1.6	12.2	19.6	42.2	51.2	41.4	*F*	17.5	92.4	91.4	*F*
	9.1	9.4	10.6	10.4	31.6	60.6	38.2	34.0	13.6	36.6	35.4	80.8
	6.4	11.2	9.4	11.2	22.7	17.4	59.4	*F*	21.5	34.0	78.4	63.4
Wi.....	4.8	4.4	10.8	8.2	25.6	30.6	38.8	14.1	25.6	28.0	24.0	56.8
	3.6	14.8	18.0	27.2	20.1	43.8	88.4	*F*	18.5	25.0	30.6	*F*
	8.2	12.8	5.6	17.2	36.6	24.4	39.2	*F*	21.6	25.2	36.6	*F*
Av.....	13.0	20.5	27.4	33.2	34.5	45.4	52.2	87.2	31.2	44.4	57.6	96.6

The value obtained at intensity *4* with accommodation plus convergence agrees very closely with that found by Wundt. Wundt found the just noticeable variation to be 1/50 the standard distance, and in these observations it is 1/58 of the standard distance. That accommodation alone is not a very accurate criterion is shown by the fact that at this intensity, which is not much lower if any than those commonly met with outside of experimental conditions, the

[1] The error values for intensity *1* would have been still greater had the range of the apparatus been wider.

[2] *F* indicates failure to make adjustments within the range of the apparatus and is assigned the error value of 120 mm. Each value is the average of five adjustments except those for intensity *4* which are the averages for ten adjustments, five at the beginning and five at the end of the series.

TABLE III

COMPARISON OF AVERAGE ERROR IN MILLIMETERS FOR ACCOMMODATION ALONE AND WITH CONVERGENCE AT DIFFERENT INTENSITIES[1]

Subj.	I–4		I–3		I–2		I–1	
	Conv.	Acc.	Conv.	Acc.	Conv.	Acc.	Conv.	Acc.
Ax	32.6	34.8	49.3	48.2	74.4	54.6	67.7	96.2
Du	13.6	26.9	24.6	48.5	25.1	48.7	36.7	80.4
Fa	7.9	32.1	19.9	56.6	23.1	49.3	40.9	73.4
Ne	11.1	54.8	11.0	37.7	19.8	76.5	22.4	*F*
We	7.4	24.8	7.4	48.7	10.7	57.4	13.7	89.7
Wi	5.5	23.0	10.7	29.5	11.5	43.0	17.5	91.8
Av.	13.0	32.7	20.5	44.9	27.4	54.9	33.2	91.9

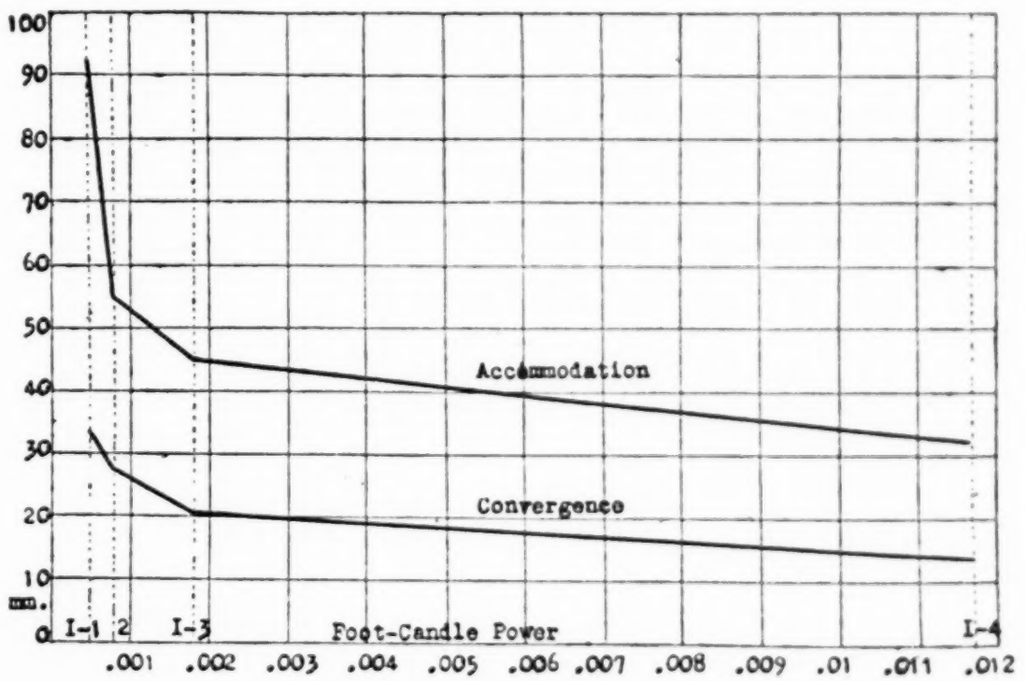

FIG. 1. Comparison of average error for accommodation alone and with convergence at different intensities of illumination.[2]

[1] This table gives a comparison of monocular (accommodation alone) and binocular (accommodation plus convergence) adjustments. The values for accommodation alone are averages of all the right-eye and left-eye readings. All of the values of Table III are averages of the results from different days as given separately in Table II.

[2] The values represented in the graph are those given as averages in Table III. They are the average error values for the whole group of observers.

average error for accommodation alone is about twice that for accommodation plus convergence.

2. *The Effects of Practice and Fatigue upon Accommodation and Convergence*

TABLE IV

COMPARISON OF AVERAGE ERROR IN MILLIMETERS AT BEGINNING AND END OF ONE-HOUR ADJUSTMENT PERIODS[1]

Subj.	Both Eyes (Convergence)		Right Eye (Accommodation)		Left Eye (Accommodation)	
	Initial	Final	Initial	Final	Initial	Final
Ax	42.3	22.0	27.3	83.3	67.0	40.0
	48.6	62.0	26.8	23.0	73.6	27.0
	3.6	20.2	42.8	18.8	22.0	22.0
Du	23.4	20.8	32.6	13.8	20.2	18.1
	15.6	13.8	38.2	28.0	26.8	36.4
	6.2	1.8	18.4	28.0	27.0	21.2
Fa	18.8	7.6	62.4	17.2	27.8	29.2
	8.2	7.0	28.6	34.6	43.0	24.8
	4.0	6.2	17.8	39.2	19.8	40.6
Ne	11.8	5.0	63.6	65.2	50.4	67.0
	4.0	7.6	34.0	70.3	17.6	45.6
	18.4	4.2	67.2	59.4	40.1	76.6
We	13.2	5.0	41.2	22.0	14.6	12.6
	2.4	11.2	18.2	66.2	18.8	68.0
	8.2	6.4	14.2	31.2	18.6	24.4
Wi	4.4	5.2	26.2	25.0	13.0	38.2
	10.8	5.6	32.0	41.2	30.0	56.6
	3.4	3.9	20.0	20.2	18.2	18.8
Av	16.2	12.5	35.9	37.6	33.6	38.2

The adjustments made with both eyes (accommodation plus convergence) show an improvement of 23 per cent. in accuracy during the hour or a decrease of 3.7 mm. in average error. On the other hand, the adjustments with one eye (accommodation alone) show an increase in the average error of 3.1 mm. or 8 per cent. Since these figures give only the end points in the curves of practice and fatigue effects, they do not warrant very definite conclusions. However, it seems safe to say that they do show that fatigue has a much quicker or greater effect upon the intrinsic (accommodating) eye muscles than upon the extrinsic (converging) eye muscles.

[1] Each of the values given represents the average for five adjustments. All values are for the highest intensity of illumination, *4*. During the interval between the initial and final adjustments about 60 adjustments were made at the other illuminations, requiring about one hour.

It should be noted that the practice factors which effected an improvement in the accuracy of the convergence adjustments were also operative in the accommodation adjustments but were more than counterbalanced in the latter case by the fatigue effects.

Retinal adaptation may possibly have some influence in bringing about an improvement during the period of an hour. At least five minutes were allowed, however, at the start of each sitting for adaptation. This time seems ample when it is remembered that the first adjustments were made at a rather high intensity. Again at the end of the period when a return was made to the highest level for a final series of five adjustments, a period was allowed for adaptation. Since adaptation is much more rapid when the new illumination is the higher, it is evident that if adaptation is operating as a factor in the results described, its operation must be in the same direction as that ascribed to practice. Hence allowance for the factor of adaptation makes the figures even more significant of a greater effect of fatigue upon accommodation than upon convergence.

TABLE V

COMPARISON OF AVERAGE ERROR IN MILLIMETERS AT BEGINNING AND END OF THREE-WEEK ADJUSTMENT PERIODS[1]

	Both Eyes (Convergence)		One Eye (Accommodation)	
	Initial	Final	Initial	Final
Ax	32.2	11.9	40.4	26.4
Du	22.1	14.7	21.2	32.4
Fa	10.7	5.1	40.0	29.4
Ne	8.4	5.8	61.6	41.8
We	9.1	6.4	37.8	22.1
Wi	4.8	3.6	25.6	19.3
Av	14.6	7.9	37.8	28.2

The results as given in Table V show that there is a very noticeable improvement in accuracy during a period of three

[1] Values are all for the highest intensity of illumination, *4*. Each is the average of ten adjustments, five at the beginning and five at the end of each one-hour adjustment period.

weeks for both accommodation alone and with convergence. The improvement is 25 per cent. for accommodation alone and 46 per cent. for accommodation with convergence. That the percentage of improvement is greater for accommodation plus convergence may be accounted for by the fact that the figures are the averages of adjustments at the beginning and end of an adjustment period and hence contain the fatigue factor already pointed out in connection with Table IV.

Summary

1. As shown by the results of a series of distance adjustments of small beads to a standard distance of 75 cm. at an illumination of .0117 foot-candle, the average error for accommodation alone is 1/23 of the standard distance and that for accommodation plus convergence is 1/58 of the standard distance.

2. As the illumination intensity is reduced, there is a gradual increase in error for both accommodation alone and with convergence until the intensity of about .001 foot-candle is reached. At a point near the lower threshold for vision, accommodation alone breaks down while convergence shows little change in the gradual rate of increase in error.

3. At the end of a one-hour adjustment period, accommodation alone shows a decrease in accuracy as compared with the initial trials while accommodation plus convergence shows improvement during the same time. This would indicate that fatigue has a greater effect upon the intrinsic accommodating muscles of the eye than upon the extrinsic converging muscles. Both accommodation and convergence show a very noticeable improvement in accuracy with practice during a period of three weeks.

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DEVICE FOR LOW-POTENTIAL CURRENT IN THE PSYCHOLOGICAL LABORATORY

BY D. A. MACFARLANE AND J. S. ROONEY

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A convenient control board has been assembled to provide current from the 110-volt D.C. or A.C. house circuit, for apparatus requiring varying amounts of power up to 15 volts and 1.2 amperes. The assembly can easily be modified to provide current above this amount.

The control board consists of a Ward, Leonard Electric Co. rheostat,[1] a lamp bank, fuses and switches mounted on a board or slab approximately 1′ x 2′. The wiring is illustrated in the accompanying cut.

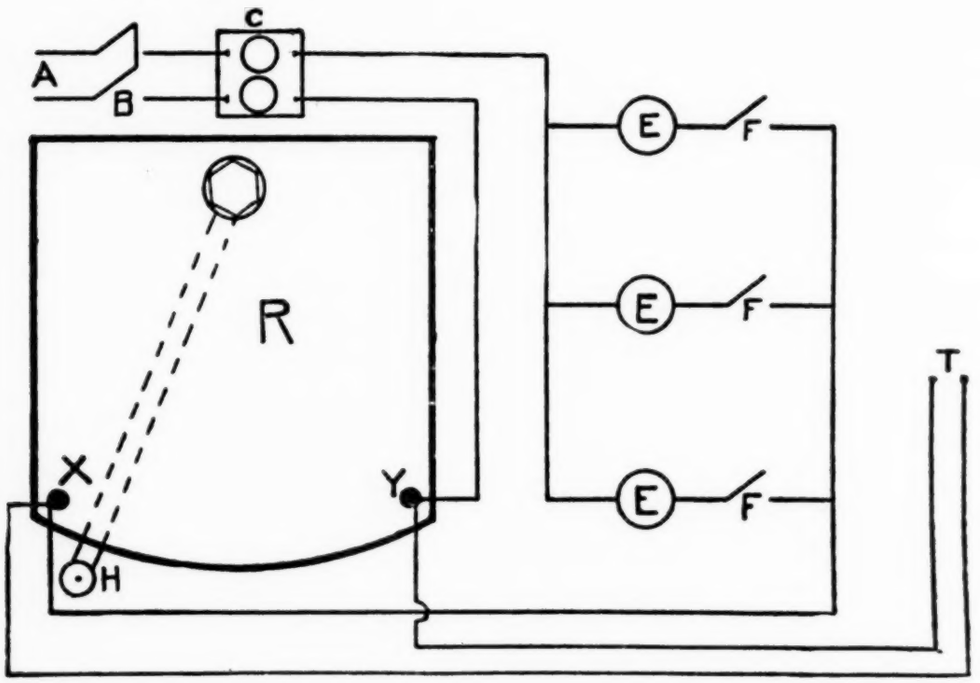

FIG. 1.

The wires from the house circuit are brought to the board (*A*), wired through a switch (*B*) and fused (*C*). From the fuse, one of these wires goes directly to one of the binding

[1] Listed as R 24 Armature Speed Control.

posts (*Y*) of the rheostat (*R*), the other passes through the lamp bank and thence to the other binding post (*X*) of the rheostat.

The lamp bank, which helps govern the range of current stated in the opening paragraph, consists of three 60-watt lamps (*E*) and a single throw switch (*F*) in series with each lamp so that either one, two or three lamps may be thrown in at will. The capacities delivered to the apparatus mains by different combinations of lamps are as follows:

1 60-watt lamp	delivers up to approximately	5 volts	.....	.4	ampere
2 " "	lamps deliver " " "	10 "	.....	.8	"
3 " "	" " " " "	15 "	.....	1.2	amperes

The mains for the apparatus are connected with the same binding posts (*X* and *Y*) of the rheostat with which the above connections are made. It has been found convenient to provide these lines with an additional set of binding posts (*T*), thus breaking the apparatus mains into two sections.

The amount of power drawn off for the apparatus is controlled by the number of lamps burning in the lamp bank and by the position of the control handle (*H*) of the rheostat. The cost of the assembly should be within $25.00.

CHANGES IN MOTOR CONTROL AND INDIVIDUAL VARIATIONS UNDER THE INFLUENCE OF 'RAZZING'

BY DONALD A. LAIRD

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I. Introduction

In the Great American Sport many games are supposed to be won or lost by the remarks of the fans. These are usually directed toward the pitcher but when the turn comes to bat no player is immune from these jibes, commonly personal to the *n*th degree and but rarely encouraging or friendly.

That these thrusts of the fans have an effect is common knowledge. The Master of Swat may go to pieces and chase the offending rooter through the stands; or he may simply go to pieces at the bat and ignominiously fan. Some stars of the diamond are so affected by these rooters' cheers and cat calls that they are put into the game principally when on a friendly field. Others seem to be benefited by the discouraging element among the rooters.

It is rather a great jump from the studies of the influences of distractions, noises, attitude of the experimenter and such on reaction times to the effects of these rooters' jibes on motor control in athletic performances. Distractions do have their influence on the time of reaction under laboratory conditions. Summaries and recent work in this field are published by Evans (1) and Morgan (2).

Less simple motor control is probably also affected by conditions of distraction and analogous situations. Definite evidence on this point, however, is lacking. The influence of noises as distractions upon the rate of tapping has been studied (3,4), but without any conclusive results. Some little effect of the beating of a metronome and the sounding

of a whistle seems to be in evidence in an irregularity of the tapping under these distractions.

A more recent study by Tinker (7) is concerned with the influence of distraction caused by ringing a bell when a stylus came into contact with the side walls of a maze. Negative results were obtained.

Obviously such a simple distraction as the beating of a metronome or the ringing of a bell upon failure in a certain test is rather far removed from the consistent 'razzing' of a group of so-called sports. The present study is concerned with the effects of such complicated variables as enter into 'razzing' upon performance, especially as measured by certain common tests of motor control. In anticipitation it may be said that the effects of such a distraction as this are much more pronounced than the equivocal results obtained with bell-ringing and color-flashing.

II. The Experiment

A series of so-called motor tests was given to a group of fraternity pledges under two different conditions. First, the tests were given under conditions of friendly competition. Active members of the chapter as well as the other pledges watched the performance of each pledge on the motor tests to be described. During the active performance of each test respectful silence was observed, just as if it had been a goal kick in progress rather than such a simple affair as tapping. Each pledge tried to do his best; each one tried to outstrip the others. The spirit of all was excellent.

Two nights later the same tests were given under similar conditions except that this was by organization of the actives turned into a 'bull' session as well as simply testing. In place of taking a quiet, sincere, interest in the performance of each pledge as they had on the previous occasion, this time the actives proceeded to 'razz' the pledges as it came the turn of each to go through the texts. Much of this 'razzing' was intensely personal; much of it was merely discouraging remarks. All in all it was very effective, so much so, in fact,

that two records had to be discarded due to errors on the part of the experimenter who became unduly engrossed in the patter.

The following tests were given the subjects:

Test 1. Tapping Speed.—This test was modified after the procedure given in Whipple (5). The stylus and tapping board described by him were used. An electric torque counter specially designed so that it would record twenty taps a second without error was used rather than a kymograph Records were kept for ten second periods. The periods were separated by a two second interval during which the record of the preceding ten second interval was taken. The length of the intervals was controlled by the disc commutator time control instrument (6).

Records were kept for four such intervals in immediate succession. In all forty-four seconds tapping, but only records of what was done during forty of them. A two seconds interval was allowed for warming up before the record commenced.

The fatigue index for speed of tapping was calculated (5).

Test 2. Three-Hole Test.—This was a modification of the test used by Hollingworth in his study on caffein. Mechanical counters were used at the bottom of each hole. One minute was the time allotted.

The records given in this report include only the numbers punched on two of the holes. The third hole developed counter difficulties under the second part of the experiment when the subjects were 'razzed.' This difficulty was caused by one of the subjects breaking the counter attachment. This gives us a clew to some of the effects of 'razzing' that were not studied directly in this experiment, namely, increased force of movement. This also manifested itself in one of the subjects breaking the end off the tapping stylus under 'razzing.'

Test 3. Steadiness, Sitting.—This test, as described by Whipple (5), was given sitting. Only one hole was used in order to economize on time. This was the second from the left on the lower row. Three seconds was allowed for the

subject to get position and then the counter was switched into contact. Readings were made at the end of fifteen and thirty seconds. The height of the instrument was adjusted so that the base was on a level with the inside curve of the elbow joint. The stylus was held at right angles to the plane of the opening.

Test 4. Steadiness, Standing.—This test was the same as Test 3 except that the subject was standing and the instrument was adjusted for this altered height. In both steadiness tests the subject held his elbow close to his body.

The tests were given in the order in which they are numbered. Each pledge was given a number, which indicated the turn in which he took the tests. Test 1 was given *all subjects* first then Test 2, and so on. This allowed a period of rest in which fatigue could be offset, and incidentally made for speed in testing.

During the second part of the experiment in which the subjects were 'razzed' they were taken one at a time, those not being tested remaining in a separate room until their turn came. Each subject was instructed in this part of the experiment not to tell any of the others what transpired in the smoking room in which the tests were conducted. At the close of the experiment each pledge confessed that he thought he was the only one being 'razzed.' Some of them were distinctly 'sore' and on the verge of handing over their pledge pin until they found that they had been the victims of psychological vivisection.

III. Results

Table I. gives the standing of the eight individuals in the tests indicated, under apparently normal conditions. For the tapping test only the total number of taps is given together with the fatigue-index, which was computed by subtracting the last 10 second record from the first 10 second record and dividing this by the record for the first 10 second interval. It will also be recalled that the record for the three-hole test is that of only two of the three counters, due to mechanical difficulties.

TABLE I

STANDING OF SUBJECTS IN THE TESTS UNDER NORMAL CONDITIONS

Subject	Test				
	Tapping		Three hole	Steadiness	
	Number	Fatigue Index		Sitting	Standing
A	257	.09	87	9	30
B	256	.09	92	12	22
C	235	.12	74	5	7
D	222	.13	85	0	7
E	246	.18	86	3	7
F	259	.21	76	6	2
G	237	.19	92	7	13
H	262	.27	89	9	37

A cursory examination of the data reveals a lack of correlation between the rank of each individual in the various tests given. The fatigue-index for tapping, for instance, is greatest for the individual who tapped the most in the time alloted, although the subject with the smallest fatigue-index ranks third in speed. This low correlation between the various motor abilities tested, however, is aside from the main part of the problem, but should be mentioned in passing.

TABLE II

STANDING OF SUBJECTS IN THE TESTS UNDER 'RAZZING'

Subject	Tests			
	Tapping	Three-hole	Steadiness	
	Number		Standing	Sitting
A	262	98	14	29
B	263	108	17	24
C	239	88	9	13
D	228	82	8	17
E	249	84	9	14
F	256	75	10	8
G	232	87	14	25
H	248	80	21	57

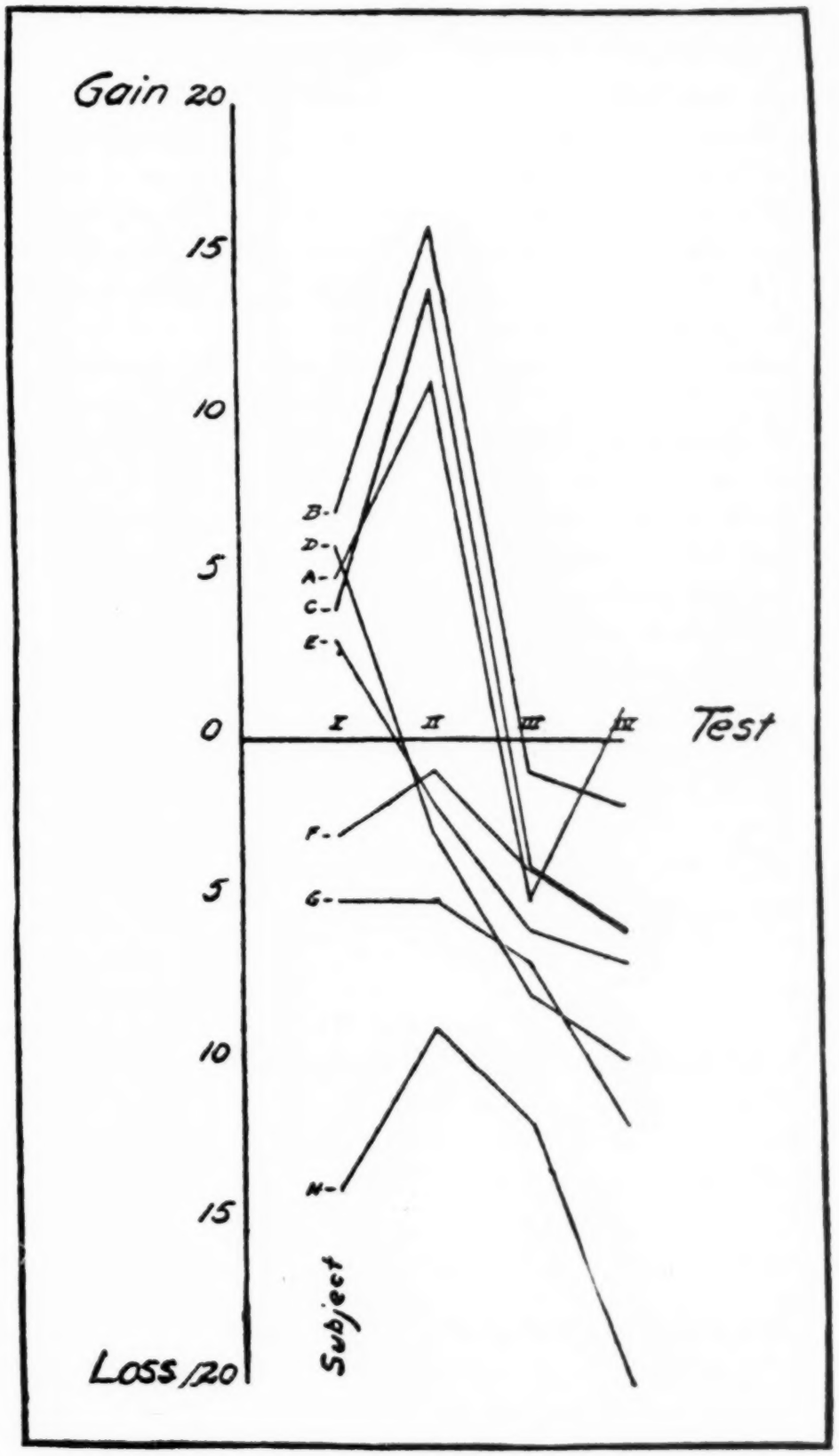
Gain 20
15
10
5
0
5
10
15
Loss 20
I
II
III
IV
Test
B-
D-
A-
C-
E-
F-
G-
H-
Subject

CHART I.

More important for our purposes is the comparison of the motor accomplishments of these subjects under seemingly normal conditions with the accomplishment on the same tests a week later under conditions of 'razzing.' Table II. gives the data for this second testing. In Chart 1 is given the gain or loss of each subject on each test indicated under the 'razzing.' A higher record in tapping or in the three-hole test constitutes a gain, while a higher record on either of the steadiness tests constitutes a loss.

Practically all of the subjects lose in the steadiness tests under 'razzing,' while the disturbance seems to enable about half of them to do better in tapping and the three-hole type of coördination. Whether or not these constitute real gains is a question, since the factor of improvement through limited practice has not been eliminated. About the losses, however, there can be no question of their being a real loss.

On an absolute basis the loss under 'razzing' is greater for the steadiness standing, in which there is steadiness of both trunk and arm muscles, than for the steadiness sitting. When reduced to a basis of per cents., as is done in Table III.,

TABLE III

LOSSES AND GAINS IN THE STEADINESS TESTS UNDER 'RAZZING,' IN TERMS OF THE PER CENT. OF ORIGINAL PERFORMANCE

Subject	Sitting		Standing	
	Loss	Gain	Loss	Gain
A	55			3
B	42		9	
C	80		86	
D	800[1]		143	
E	200		100	
F	66		300	
G	100		92	
H	133		54	
Average	184		97	

this difference is found to go in the opposite direction. But even on this basis if individual *D* were eliminated there would

[1] Original record taken as being one in calculation to avoid dealing with ∞.

still be a greater relative loss in control in standing than when seated so far as steadiness is concerned. This may be indicative of 'razzing' affecting the control of the larger and coarser musculature in most subjects more markedly than the arm muscles are affected. It may also account in part for the lack of striking changes in the other tests used, which were concerned in the main with control of the arm muscles.

For all of the subjects steadiness is affected most and in an unfavorable way. For most of the subjects the three-hole coördination test shows a lessened performance under the second conditions. The actual results of the tapping test show increased performance for most of the subjects, although this, as has been suggested, may be more apparent than real.

Marked individual differences are readily apparent. Three of the subjects went to pieces on the steadiness tests only; five of the subjects went to pieces on the steadiness tests and the coördination test; only three subjects suffered diminished performance on all of the tests of motor control.

The question naturally arises to the practical person: How can these varying degrees of loss of control under 'razzing' be predicted. This is really an unexpected by-product of the main experiment but is readily seen from the records. The subjects are arranged in Table I. in the same order that they are seen to suffer increased loss of control under the distractions used. With only one or two exceptions this is also seen to be the order of increasing fatigue in tapping as indicated by the fatigue-index.

Although the steadiness tests show up loss of control in the most pronounced way there is nothing about the performance in these tests under normal conditions to indicate whether or not the individual will be seriously affected by 'razzing.' The fatigue-index for the steadiness tests in which the records for the first 15 and the second 15 seconds were compared gives no indication whatsoever of prognosticating loss of control in the second part of the experiment.

The composite scores of all the subjects on the Thorndike Intelligence Test for High School Graduates were available. These are listed for each subject in Table IV. There is seen

TABLE IV

THORNDIKE SCORES OF SUBJECTS

Subject	Score
A	58
B	49
C	87
D	66
E	52
F	56
G	92
H	50

to be but little relation between the score and the likelihood of the person to go to pieces in motor control under 'razzing.'

If the subjects had represented a greater dispersion in intelligence it is possible that a significance might have been found in connection with this trait, but for the present group of selected subjects, there is little more than a zero relationship.

The estimates of the fraternity brothers was obtained regarding the probable modification of the records of each

TABLE V

ESTIMATES OF ACQUAINTANCES REGARDING MODIFICATIONS OF MOTOR CONTROL OF THE SUBJECTS UNDER 'RAZZING'

Subject	Number of estimates that he would			
	Not be affected	'Go to Pieces'	Do better than Normal	Judge Undecided
A	1	7	6	4
B	0	9	5	4
C	2	8	3	5
D	2	10	6	0
E	1	11	4	2
F	0	14	0	4
G	1	12	2	3
H	2	13	3	0

of the subjects under 'razzing.' They were to list under the column heads given in Table V. the probable modification of each subject in the second part of the experiment. In

Table V. these records of the eighteen judges are combined, to give the group judgment.

These estimates show that some insight, but only a little, was had by the intimate acquaintances into the probable modification of motor control in the subjects when 'razzed.' It is interesting to note in passing that one judge, whose record is not included above, came closest to making the correct estimates. This was the fraternity cook, a matronly woman past middle life.

III. Conclusions

Eight fraternity pledges were given a series of motor tests under two conditions; first, normal, and second, with 'razzing' by their brothers-to-be. Comparison of these two sets of records show that:

1. In all the subjects steadiness was diminished under 'razzing.'
2. Steadiness involving body muscles as well as arm muscles is altered the most.
3. Coördination, as needed for performance in the three-hole tests suffers decrease under 'razzing' but not so much as does steadiness.
4. The rate and fatigue of tapping suffer least of all the tests given.
5. Individual differences were plainly evident, some subjects doing better in all tests in the second part of the experiment, except the steadiness test; other subjects were below par in steadiness and coördination, while only three suffered a loss in the functions examined by all four of the tests.
6. The fatigue-index of tapping under normal conditions seems to be the best indicator of the performance of the individual under 'razzing.'

The writer wishes to make acknowledgment of the aid and coöperation of Mr. Ralph Hoitsma in arranging the details of the experimental conditions and securing the subjects and their tormentors.

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